Experimentation and Interpretation

The Use of Experimental Archaeology in the Study of the Past

Edited by

Dana C. E. Millson

VISIT…

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1. Introduction

Dana C. E. Millson

In December 2008, at the annual Theoretical Archaeological Group (TAG) conference in Southampton, I organized a session which focused on the use of experimentation in archaeology. Much curiosity ensued, questioning why such a practical part of archaeology should be discussed in a theory conference. Indeed, archaeology is a field which spans two (sometimes opposing) philosophies – that of science and arts – which is best described by Jones (2002, 1) as an "archaeology of 'two cultures'." It is this dichotomy which makes archaeology a strong discipline, capable of drawing knowledge from some of the most fragmentary evidence; but it is also what has caused much confusion about what archaeology is and how it should be conducted. This has caused countless arguments and even fissures within the discipline. In the midst of all this is Experimental Archaeology, which approaches the archaeological record in a scientific way, but in reality, is inextricably tied to the humanistic interpretations we create. It is how we test our hypotheses and then progress to develop new theories, and perhaps it is for this reason the session proposal was accepted in the first place. It is of no doubt, however, that the contributors' projects, which demonstrate the interesting and novel ways in which Experimental Archaeology can be used, and the fascinating results it can offer, is what made the session so successful that, by the second half, the audience stretched into the corridor.

A Little Bit of History...

Experimental Archaeology is not a new idea, but an implement which has been used since the days of the antiquarians (Coles 1973, 13–14; Forrest 2008, 62–65). This often involved experimentation with actual artifacts that had been excavated, but some work, such as Pitt-Rivers' (1876, 382–83) study at Cissbury, Sussex, involved the construction of replicas to perform planned experiments. Pitt-Rivers reconstructed antler picks that were found on-site and used them to dig ditches of the same depth and size as those at the hillfort. The results proved Pitt-Rivers' inference correct that the ditches had been dug using the picks and that it could be done quickly, "...we had made an excavation 3 feet square and 3 feet deep in an hour and a half...it would have taken us twelve hours to form the longest gallery found..." (Pitt-Rivers 1876, 382).

However, it was not until the 1960s, particularly with the work of Louis Binford and followers of the New Archaeology, that Experimental Archaeology became established as a practice in its own right. After realizing that the culture-historical approach was not sufficient enough to give clear detail about past culture, Binford searched for new ways to approach archaeology:

> An accurate and meaningful history is more than a generalized narrative of the changes in composition of the archaeological record through time...If we hope to achieve the aim of reconstructing culture history, we must develop means for using archaeological remains as a record of the past and as a source of data for testing propositions which we set forth regarding past events, rather than a record we can read according to a set of a priori rules or interpretive principles whose application allow the skilled interpreter to 'reconstruct' the past (Binford 1968, 11).

In contrast to past theoretical frameworks, his was a discipline which did not follow a straight path where new information was simply added to the story, but more of fibrous route where, "...every time you learn something new, it impacts on everything you ever thought you knew in some sense" (Sabloff 1998, 42, quoting Binford). Since he had first been educated in the natural sciences, taking a scientific approach, particularly testing hypotheses with experimentation, was an obvious step and he introduced the use of methods from other disciplines, particularly anthropology and the natural sciences.

In the early 1980s, however, a new movement began to develop, which criticized and rejected New Archaeology and its sub-disciplines. The Post-processualists argued that science was insufficient in the study of past culture because humans do not behave like organisms and cultures vary so greatly that scientific laws cannot be applied to them the same way as they can be to biological processes. It was further argued that objectivity could not be fully achieved when studying culture, because the bias of our own cultural values would always interfere with our interpretation (Trigger 2008, 456–57; Shanks and Tilley 1987). Moreover, they argued, the New Archaeologists, or Processualists, had completely ignored the fundamental aspects of culture that could not be considered empirically, like ideology and religion (Trigger 2008, 450–51; Sabloff 1998, 91). As part of this backlash, the methods of the Processualists, Experimental Archaeology included, were largely abandoned or marginalized.

The criticism against treating archaeology solely as a science and attempting complete objectivity is valid; however, the rejection of science in archaeology was the Post-processualists' greatest weakness. The discipline had formerly been teetering on one foot in the scientific sector, with the Post-processualist movement, but it now took an equally imbalanced shift in the opposite direction. It is true that not everyone working in the field at the time took on these ideas, or even entered the debate, but the literature enforces an atmosphere of revolution where one was obliged to choose a side, and be attacked either way.

By the 1990s, the Post-processualists' points had been made and archaeology had been changed, and it was at this point the main flaws of a completely humanistic approach began to be observed (Trigger 2008, 516). Along with this was the impetus of new scientific and technological applications that could be used in archaeology to gain new forms of data to support theory (Brothwell & Pollard 2001, xix; Trigger 2008, 540). The development of residue analysis, using mass spectrometry, was underway (Evershed

et al. 1990). Improved AMS dating, allowing for margins of error in radiocarbon dates to be greatly reduced (Taylor 2001, 26), and isotopic analysis, enabling past diet to be gleaned, were increasingly used to answer new questions (Sealy 2001, 272). Moreover, the first studies, focussing on ancient DNA, extracted from bone, were done in the late 1980s (Brown 2001, 301), and the development of more sophisticated computers and programs to manipulate data made it possible to do larger-scaled projects and more intricate statistical work. And so science crept back to a prominent position in archaeology.

Today, at the end of the first decade of the 21st century, we are well beyond the explosive arguments of the Processual/Post-processual debate, and are in a time which is more optimistic than ever before. We have inherited the knowledge of both movements and are able to look back at the merits and weaknesses of each, but now have the freedom to choose how to approach archaeological problems without having to choose one side of an ongoing argument.

Experimental Archaeology

It is at this point in history, then, that Experimental Archaeology should be able to become a working part of archaeology, yet it still holds a marginal role. Part of the reason for this is the lack of understanding by the wider archaeological community of what Experimental Archaeology actually is, what it can be used for, and why it is so important in the balance between science and theory.

Experimental Archaeology is a process whereby controlled experimentation is used to answer specific questions. This can take two forms:

1) experiments to test hypothesis made about a site or type of artefact,
2) experimentation to test methods which are used to gather data about the past to ensure the data collected is a true representation of the past.

This is vitally different from Experiential Archaeology as it attempts to gain facts from the past as free as possible from modern biases. Experience does not meet this aim because a person's experience replicating past action will always be modern. They cannot escape their own *habitus* and the bias it will have in their interpretation of past human motives and emotions (Cunningham *et al.* 2008, vi; Reynolds 1999, 158). This is not to say there is no value in experience as it can greatly help us learn and put the humanity back into the ancient people we study as we try to emulate their daily routine or how they did things. It is simply that Experimental Archaeology is a different process with different objectives. Experimental Archaeology attempts an objective approach targeting specific questions and resulting in data which can be empirically understood. However, it is linked with experience as it, "...supplies the confirmed material of and for education and experience" (Reynolds 1999, 157).

With Experimental Archaeology, a scientific methodology must be adopted where variables are controlled to ensure replicability. This may include using professionals to recreate objects which will be experimented upon (since past people would have been experts in their craft), and using materials as similar as possible to those used in the past. Each part of the experiment must be meticulously recorded so that it is

replicable (and therefore falsifiable), and it should be of a standard that is acceptable to experts working in the scientific field the application has been taken from. For example, if residue analysis is being conducted, the experiment should meet the standards of chemistry, and the paper should be peer-reviewed by chemists as well as archaeologists (Mathieu and Meyer 2002; Outram 2008). The results must then be related to the wider archaeological context so that the information is transferable and comprehensive throughout the discipline.

The Role of Experimental Archaeology

Experimental Archaeology thus forms an essential step in archaeological endeavour whereby hypotheses and theories that have been made about the past can be tested to be confirmed or rejected. Theory can then be reconsidered in light of this new information and a new foundation for further study created. So, although scientific in practice, Experimental Archaeology is strongly connected to theory and plays a bridging role between data and theory – between science and arts. It is a step where "interpretation [is] a process of creative tension in which interpretations arise from the movement back and forth between theory and data" (Jones 2002, 37). In the climate of archaeology today, where we can choose how much science or theory is necessary, based on each situation, Experimental Archaeology has a very important role to play.

The Purpose of the TAG Session

So this is why I held an Experimental Archaeology session at TAG. Because you cannot study the arts without the sciences in archaeology. Because you cannot talk about theory unless you have the data to back it up. Because theoretical archaeologists and scientific archaeologists need to communicate more. And because Experimental Archaeology needs to claim its place within the field, rather than simply being an after-thought in projects, or a marginal practice. Experimental Archaeology can be a greatly informative tool in archaeology and needs to be utilized much more in general archaeological enquiry – a recent debate at the Scottish Neolithic panel workshop about whether timber halls were thatched, or could even have possibly been so, is a good example why. The session at TAG was meant to stimulate this and draw attention to the good work that has been going on and will continue to do so. In the end, whether we are archaeological theorists or archaeological scientists, or somewhere in between, we are on the same side and are all seeking the same thing: "...the evidence and experience of life, in the hope and the knowledge that by doing so mankind will better understand why life is as it is, and why man behaves as he does" (Coles 1979, 1) – we seek the answer to the 'why's' of the human condition.

Contributions to this Volume

The papers presented in this book cover several areas of experimental archaeology, including its history, philosophy and the wide range of ways it can be used to test our

hypotheses about the past. In chapter 2, Penny Cunningham considers storage pits, which are found in great numbers on Mesolithic sites. Her experiments test whether different types of pits were more favourable than others for the storage of nuts. Since Mesolithic sites tend to be composed mainly of pits and charred nut shells, the result of this work is a better understanding of site formation and use, and therefore, will enable better classification in future excavation.

Jodi Reeves Flores provides an in-depth look at the history of experimentation in archaeology in chapter 3, extending from the very beginnings of the discipline to present, and provides insight into just how much experimental work is currently going on.

In chapter 4, Claire Marshall, a professional musician, studies sound at Neolithic monuments, using instruments she constructed out of materials that would have been plentiful in the Neolithic, and from faunal analysis at sites, are known to have been important resources.

Chapter 5 focuses on the philosophy behind experimentation and how it fits into archaeological enquiry. Stephanie Koerner considers the ideas of several thinkers and, for the first time, applies their concepts to archaeology, to create a new foundation for experimentation.

In chapter 6, Merryn Dineley discusses the continuing project that she has been working on, considering the possibility that the largest Grooved Ware vessels may have been used for beer brewing. This is particularly intriguing since it is most likely that new forms of pottery were developed for their function or contents, rather than simply for aesthetic purposes. The introduction of beer has great implications for both cultural change and for a greater use of domesticated plants in the Late Neolithic.

Chapter 7 covers Harriet Hammersmith's research on Beaker pottery and the manufacturing processes, which were employed to create the different types known in the literature. As a professional potter, Hammersmith reconstructed replica vessels using different methods that have been inferred in previous publications by Beaker experts. Her results largely prove these hypotheses wrong, and give alternative ways in which Beakers may have been constructed.

Finally, Thomas Gurling's work, presented in chapter 8, is the secondary type of experimental archaeology outlined above, where a scientific application is tested so that it can be more accurately applied to archaeological remains. Here, luminescence dating is tested, considering brickwork from Medieval buildings of an approximated age, and is evaluated for its usefulness as an archaeological method.

The contributions to the session held in Southampton reflect the diversity of work that is currently being done in experimental archaeology, but also just how pertinent this methodology is to the wider archaeological context. Although it is not a new concept, it is now a new time when this method of testing can be less controversially used to check the validity of our interpretations and further our understanding of the past. It is with this momentum that we, as archaeologists, should grasp this opportunity to use all that is accessible to better understand the past.

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I would first like to thank all of the contributors for making the session at TAG08 so successful and so very interesting. Equally, special thanks to University of Southampton and the co-ordinators of TAG for accepting the session in the first place and for providing the equipment and space needed. A personal thank you goes to my PhD supervisors, Prof. Chris Scarre and Prof. Peter Rowley-Conwy, who are an unending support to me, and whose encouragement and advice helped to make this possible. And also to my family and friends who are eternally supportive of my endeavours, no matter how ambitious they may seem.

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2. Cache or Carry: Food Storage in Prehistoric Europe

Penny Cunningham

Abstract

Ethnographic evidence demonstrates that both hunter-gatherers and farmers practice a wide variety of storage methodologies. Having both practical and social implications, the use of caches appears to be particularly significant. Nevertheless, there are few discussions exploring the use of storage, beyond the storage of grain, during European prehistory. Through a series of hazelnut and acorn storage experiments, this paper explores the use of storage during prehistory. The experiments are based upon ethnographic and archaeological evidence of pits and nut macro remains from a number of sites in Europe and tests the suitability of storing nuts in pits for 18–24 weeks, using three different methods. The results from the experiments present a re-evaluation of our understanding and interpretation of prehistoric storage practices. We learn that the small-scale storage of nuts in caches may have been a vital and important mechanism enabling Mesolithic people to remain mobile and that during later periods, the storage of nuts was also significant.

Introduction

Generally, when we think of food storage in European prehistory, we typically think of the storage of large quantities of grain in either pits or granaries during the later prehistoric periods (Cunningham 2008, 295). This is largely due to a misconception that Mesolithic hunter-gatherers had no need to store food and that storing food would have prevented or restricted mobility (Binford 1980, 5; Woodburn 1980, 99; Testart 1982, 524; Halstead and O'Shea 1989, 4; Rowley-Conwy and Zvelebil 1989, 44; Richmond 1999, 6; Mithen 2000, 385–388, 618, see Cunningham 2008 for a more detailed discussion). In contrast, when people started cultivating crops and living a more sedentary lifestyle, storing cereals became a vital risk buffering strategy to ensure that there was enough grain to consume later and for the following year's crop. Furthermore, storage by early farmers would have involved the use of complex preservation techniques and large immoveable facilities, all of which would have little use to mobile hunter-gatherers

(Binford 1980; Testart 1982; Halstead and O'Shea 1989, 3; Rowley-Conwy and Zvelebil 1989, 44; Smith 2003, 168).

Although the use of storage is considered a practical and necessary practice during later prehistory, and despite the archaeological evidence of nuts in storage pits and in ceramic vessels within pits, there are few discussions regarding the implications of such storage methodologies. Most discussion concerning the use of storage during these later prehistoric periods focus purely on the practical and social implications of storing grain; few consider the importance of storing other resources (e.g. Thomas 1999). The lack of evidence of storage, particularly in the form of recognisable storage pits, during the Mesolithic, present problems when trying to identify the use of storage during this period. Thus, we are left with only limited discussions regarding the practice of storage in European prehistory (see Woodman 1985; McComb 1996; Richmond 1996; Thomas 1996; Vencl 1996; Mithen 2000; Robinson 2000; Sieso and Go'mez 2002). However, as highlighted by Rowley-Conwy and Zvelebil (1989, 45, 51) other forms of indirect evidence including resource specialisation, permanent settlements and mass capture technology may indicate that storage was practiced.

This paper proposes that by using ethnographic data, the archaeological record and storage experiments, we can begin to expand our understanding of the use of storage in prehistory. In addition, we can identify the type of evidence that signifies the use of storage in the archaeological record and begin to recognise that storage has both social and practical implications throughout prehistory.

Firstly, I will briefly outline some of the evidence of storage from ethnographic and ethnohistoric accounts, and the archaeological record, before presenting the aims, methodology and results from a series of acorn and hazelnut storage experiments. Thirdly, I will use the results from the experiments to reassess the archaeological evidence. In the conclusion, I suggest that storage, in the form of caches, would have formed an important, and one could say vital aspect of Mesolithic lifeways and that during the later prehistoric periods, the storage of non-cereals was equally important to the storage of cereals.

Ethnographic and Ethnohistoric Accounts of Storage in Pits

Ethnographic literature documents the use of storage by both mobile and semi-sedentary hunter-gatherers (Table 2.1). Although the evidence comes largely from more extreme environments than temperate Europe, in all the different environments, including temperate Europe, there is a reliance on seasonal resources. The important role that seasonal resources play in the diet of both semi-sedentary and mobile hunter-gatherers is a significant factor when considering the use of storage.

The evidence demonstrates two very broad methods of storing plant food by hunter-gatherers whether mobile or semi-sedentary: above ground in granaries and caches or below ground in pits also called caches. The use of caches appears to be particularly significant, and is not simply a method of storing resources for later consumption, but also a method of hiding or displaying stored resources and as a means of delaying and controlling consumption. Above ground caches can also be used as territorial boundary markers and as a means of landscape enculturation. Hidden caches, such as pits, may

Region	Resource	Storage Method	Storage Location	Reference
California*	Acorns	Pits (lined and unlined), baskets, Granaries	Settlement, gathering sites	Stevenot 2004, 1; Heizer & Elsasser 1980, 99; Howes 1948, 174; Spier 1978, 428; Mason 1992, 67: Saunders 1920, 7–72
Canada	Meat, Plants	Caches – platforms, beneath stone piles	Settlement, kill sites, along route ways	Stopp 2002, 314; Balikci 1968, 81, and Rowley-Conwy and Zvelebil 1989, 48
Greenland	Meat	Caches – platforms, beneath stone piles	Base camps, kill sites, along route ways	Eidlitz 1969, 98; Grønnow *et al.* 1983, 45; Odgaard 2008, 26

*Semi-sedentary hunter-gatherers

Table 2.1. Hunter-gatherer storage methods from several regions.

indicate the hiding of a surplus, and as a means in which to avoid sharing resources with the wider community (Cunningham 2008).

Interestingly, we also find a variation in the location of storage facilities including along route ways (Balikci 1968, 81; Rowley-Conwy and Zvelebil 1989, 48), at gathering/kill sites (Mason 1996; Stopp 2002) and/or at settlements (including base camps) (Saunders 1920, 7–72; Spier 1978, 428; Heizer and Elsasser 1980, 99; Grønnow *et al.* 1983, 45; Mason 1992, 67; Stevenot 2004, 1; Odgaard 2008, 26). There are also a number of reasons why groups store food including when there is an abundant harvest and for a period of known food shortage. In addition, caches were used to ensure there were supplies when returning to a location at a time when other foods maybe in short supply or for when passing through an area on seasonal rounds (Stopp 2002; Cunningham 2008). The types of resource stored by hunter-gatherers and farmers are those that store well, such as processed meat, fish and/or those that are seasonal and produced in abundance including nuts and fish.

As we can see, far from making mobility difficult, we can suggest that storage enables hunting and gathering. The ethnographic evidence of storage clearly indicates that storage is not simply about 'saving it for later'; there are social as well as practical implications for the methods used for, and the reasons behind, food storage (Ingold 1983).

Archaeological Evidence

Despite the evidence of a variety of pit storage methods used to store acorns and hazelnuts from the Neolithic onwards in Europe, there are very few discussions of the significance of storing non-cereal resources (Table 2.2). Thomas (1999, 64, 74) believes that pit morphology from British Neolithic sites is so different from the Iron Age sites that they cannot be intended for storage and that pits specifically designed for the storage of grain, first appeared at the same time as substantial settlements and fields in the Late Bronze Age. Therefore, he argues that no preceding pits were used for storage. By demonstrating that grain stores best in bell-shaped pits, the results from Reynolds (1974) grain storage experiments support Thomas' (1991) contention. However, Reynolds (1974) based his experiments on the shape and size of pits at Iron Age sites and he never attempted to store any other type of food stuff than grain.

Thomas (1999, 64) also believes that the method in which pits were backfilled can be used to indicate the likelihood of their use as storage pits. The stratigraphic evidence from Iron Age sites such as Gussage All Saints and Little Woodbury, England, indicates that the pits were backfilled slowly with rubbish. The slow backfilling signifies that they were originally storage pits, and subsequently used as a rubbish dump, whereas smaller Neolithic pits appear to have been backed filled swiftly with intentionally placed soil and objects, and therefore had other functions. I am not sure why a pit that was purposely backfilled with soil and had objects placed in it could not have been previously used as a storage pit. The problem is that Thomas (1999) is equating storage with grain only; consequently, there is no consideration that other foods may have been stored in pits during the later prehistoric periods.

In contrast, the evidence of nut storage demonstrates that they were stored in a variety of different shaped pits and sometimes within pots in pits (see Table 2.2). Therefore, the methods used to store nuts might be varied and not as obvious in the archaeological record as grain storage pits. Evidence of nut storage from Jomon sites in Japan also indicate that the shape and size of storage pits differs; thus, there does not appear to have been a standard storage methodology (Habu 2004, 66–67). However, what is not clear is whether environmental factors influenced the difference in storage methodology in Japan or whether we need to also consider cultural factors.

The evidence for pit storage in the Mesolithic is difficult to identify, but this is partly due to the problem of recognising the function of pits, but as the evidence of nut storage from later prehistoric periods indicates, rather than dismissing the use of pit storage, we can consider the presence of some pits as, perhaps, signifying storage. Nevertheless, this is not to say that every pit found in a Mesolithic context was used for storage, we have to also consider other factors such as the location of the site in relation to seasonal resources that are also produced in abundance and the suggested use of the site.

Possible evidence of nut storage in the Mesolithic comes from three sites; Lough Boora, Ireland (Ryan 1980; McComb and Simpson 1999, 10), Mount Sandel, Northern Ireland (Woodman 1985) and Staosnaig, Colonsay (Mithen 2000). At Lough Boora (8980–7475 BP), a small cache of 487 uncarbonised whole hazelnuts was found in a shallow pit; within the vicinity of the pit were hearths, burnt animal bones and microliths. The

Site Name	Date	Context	Pit Dimensions	Nut Species	Reference
Mount Sandel, Northern Ireland	9538–9305 BP	Large, straight sided storage pit (F56)	Depth 40–50 cm Diameter 100 cm	Hazelnuts	Woodman 1985
Lough Boora, Ireland	8980–7475 BP	Shallow pit containing uncharred hazelnuts	Diameter 30 cm	Hazelnuts	Ryan 1980, McComb and Simpson 1999
Staosnaig, Scotland	8000 BP 7500 BP	Stone lined pits	F43 – diameter 76 × 58 cm, depth 18 cm F49 – diameter 98 × 70 cm Depth 20 cm	Hazelnuts	Mithen 2000
Verrebroek 'Dok 1' Belgium	Mesolithic	Two storage pits	Pit S80 – depth 60 cm Pit S89 – depth 48 cm	Hazelnuts	Sergant *et al.* 2006
Dog Farm, England	Neolithic	Pit storage of hazelnuts	-	Hazelnuts	Robinson 2000
Pègue, France	Neolithic	Storage pit containing acorns and barley	-	Acorns	Vencl 1996
Fiavé, Italy	1400–1200 bc	Storage pits containing acorns and hazelnuts		Hazelnuts and acorns	Jones and Rowley-Conwy 1984
Buraco da Pala, Portugal	2800–2200 bc	Rock shelter, granary and storage vessels	-	Acorns	de Jesus Sanches 1996
Cave of Nerja, Spain	3442–3221 BP	Large underground silo lined with limestone slabs contained a mixture of barley, wheat and acorns	-	Acorns	Sieso and Go'mez 2002
Cave of the Toro, Spain	Bronze Age	Large ceramic vessel contained acorns, wheat and barley	-	Acorns	Vencl 1996
Dortmund-Oespel, Germany	3319–2958 BP	Peeled acorns in pits	-	Acorns	Vencl 1996

Table 2.2. Archaeological evidence of nut storage.

Site Name	Date	Context	Pit Dimensions	Nut Species	Reference
Dorset, England	Bronze Age	Pit containing charred acorns	-	Acorns	Ladle 1998
Hamburg-Boberg, Germany	Bronze Age	Peeled acorns in pits	-	Acorns	Vencl 1996
Flögeln, Germany,	Bronze Age	A pottery vessel contained peeled acorns	-	Acorns	Vencl 1996
Amarejo, Portugal	Bronze Age	A pottery vessel contained acorns, wheat, barley and almonds	-	Acorns	Vencl 1996
Evergern-Ralingen, Belgium	2732–2325 BP	Stored acorns in a silo that was almost rectangular in shape	Diameter 1.65 × 1.15 m Depth 50–100 cm	Acorns	de Ceunynck 1991
Son en Breugel, Netherlands	2460–2250 BP	Pit containing grain and charred acorns	-	Acorns	Vencl 1996
Boezingo, Belgium	2350–2169 BP	A large oval shaped pit containing an estimated 47,000–71,000 charred acorns (stored after roasting?)	Diameter 2.3 × 1.75 m	Acorns	Deforce *et al.* 2009
Amersfoort-Noord, Netherlands	Iron Age	Pit containing acorns	Diameter 70 cm Depth 30 cm	Acorns	Cramer 1990, Vencl 1996
Colmschate, Netherlands	Iron Age	Pit containing acorns	-	Acorns	Vencl 1996

Dates calibrated using the Oxcal 4.0 program (Bronk Ramsay 1995, 2001)
Interpretation of pits as storage pits is based upon the content of the pits, presence of whole nuts, and whether they have been interpreted as possible storage pits in the literature.

Table 2.2 contd. Archaeological evidence of nut storage.

quantity of hazelnuts was too large for squirrels, or any other nut-burying animal to cache, and therefore must be the result of human action (McComb and Simpson 1999, 10).

Woodman (1985, 129–136) interprets pit F/56 from Mount Sandel, as a hazelnut storage pit. He bases his interpretation on the size and shape of the pit and the presence of charred hazelnut shell fragments and the location of the pit within one of the hut

structures. Mount Sandel also has evidence of a range of activities, including lots of charred hazelnut shells, possible meat and/or fish drying racks and hut structures. All the evidence indicates semi-sedentary occupation at Mount Sandel where the Mesolithic inhabitants caught fish, gathered hazelnuts and then processed them on site along with other resources (Woodman 1985, 168).

At the site of Staosnaig a large pit feature (possible house pit) contained charred hazelnut shell fragments representing an estimated 300,000 hazelnuts (the largest hazelnut assemblage found to date in northern Europe). Near to this feature were three smaller pits that Mithen (2000) suggested might have been used for hazelnut storage. However, in his overall interpretation of the Mesolithic activity on the island of Colonsay, he dismisses the use of storage at Staosnaig because he believes that island resources were utilised by foragers who did not practice storage. Instead, he believes that Staosnaig was a hazelnut-processing site and the three pits may have been used to roast the hazelnuts.

These three examples possibly indicate different uses of storage in the Mesolithic. The evidence from Mount Sandel is indicative of semi-sedentary occupation, possibly seasonal or year-round by different groups, whereas Lough Boora could be interpreted as a temporary site, perhaps situated along an important lakeside pathway, where a cache of nuts were stored for a return visit to the site. At Staosnaig the large hazelnut assemblage, believed to have been gathered on two occasions (Mithen 2000) is such a large assemblage that storage may have been a means in which to overcome the problem of transporting the nuts to a settlement.

Apart from the clear evidence of storage at Lough Boora, the evidence from Staosnaig and Mount Sandel is tentative and their interpretation as possible storage pits are based upon a combination of factors including large quantities of hazelnuts, pits, the presences of hazel near to the sites and the suggested use of the site.

To further understand the use of nut storage in prehistory and to determine whether it is possible to distinguish storage pits from other pits, we have to determine how suitable hazelnuts and acorns are for pit storage, how long one can store them for and whether they need any form of processing to aid successful storage. In addition, is there a storage methodology (i.e. pit size and shape) that aids successful nut storage and one that will also help identify the presence of nut storage pits in the archaeological record?

To investigate the storage potential of nuts using methods and materials that would have been available in the prehistory, I stored hazelnuts and acorns in both deep cylindrical and shallow bowl-shaped pits, during two years: 2004–5, 2005–6. Archaeological, ethnographic and ethnohistorical evidence and the results from previous storage experiments (Reynolds 1974; McComb 1996; Cunningham 2002, 2005) informed the experiment methodology.

Hazelnut and Acorn Pit Storage Experiments

A number of methodological issues, including the capping material and method and whether to use a pottery container or to line the pit, needed further clarification before undertaking the experiments.

There are examples of grain and nuts stored in pottery vessels within pits from various regions of Europe (Field *et al.* 1964; Sakellaridis 1979; Phillips 1975, 106; Sieso and Go'mez 2002, 5; Table 2.2) and McComb (1996, 85) successfully stored hazelnuts in a clay pot. Suitable lining material includes basketry, leather and pottery sherds (Field *et al.* 1964, 373; Barclay and Russell-White 1993, 64; Thomas 1999, 65; Miyaji 1999, 5). Basketry lining and baskets were also used in nut storage pits at Jomon sites in Japan (Miyaji 1999, 165, 167; Habu 2004, 66–67).

McComb (1996, 85) also found storing hazelnuts in a basket and then in a pit proved a successful method of storage. Reynolds (1974, 123, 127) lined some of his grain storage pits with basketry, which proved a successful method for storing grain, but the lining could only be used once.

In addition, there is very little archaeological evidence of the materials and methods used to seal storage pits. Storage pits from Winterbourne Dauntsey, England had stake holes around the edges of the pits indicating some sort of capping structure (Field *et al.* 1964, 373). Evidence from pits at Balfarg, Scotland, also hints at a possible capping method: a number of storage pits had a layer of stones at the top of the pit (Barclay and Russell-White 1993, 167). McComb (1996, 69) also used stone capping in her storage experiments. Reynolds (1974) found that sealing the pit with a layer of clay created anaerobic conditions that allowed the grain to successfully store. Historical evidence suggests that using sand to line the sides and base of a bowl-shaped pit and as a capping layer is a suitable storage method (Loewenfeld 1957, 40; Howes 1948, 184), but when McComb (1996, 85) used this method, she found that only 47% of the hazelnuts were edible after storage.

The archaeological evidence of storage outlined in Table 2.2, the ethnographic evidence and the results from previous experiments (McComb 1996; Cunningham 2002, 2005) influenced my decision to use very simple methods of capping, to store some of the nuts in baskets, and to use both bowl and cylindrical shaped pits for this series of storage experiments.

Experiment Aims

- Test the suitability of hazelnuts/acorns for pit storage.
- Test whether nuts store better in baskets within pits.
- Test whether there is a time limit in which hazelnuts/acorns can be stored in pits.
- Test whether hazelnuts/acorns store better in deep cylindrical or shallow bowl shaped pits.
- To test whether hazelnuts/acorns were a suitable food resource to leave in caches.

Storage Methodology

The nuts were stored in 12 pits for 20–32 weeks, over two years (2004–5 and 2005–6) at the same location in Devon, England. As with most actualistic experiments there were some uncontrollable variables, mainly ground and air temperature, and rainfall, that influenced the results. In addition, a number of controllable variables including the use of baskets and different shape and size pits were implemented to test different storage methods. In the second year, some of the nuts had also been roasted before storage. The pit dimensions were roughly the same size for each storage method and similar quantities of nuts were stored in each pit (3–4 kg). Although the duration of storage did differ, storage began in October (hazelnuts) and November (acorns) each time. Unless roasted, the nuts were dried for four weeks before storage. These slight changes to the methodology implemented during 2005–6 experiments were based on archaeological evidence and the continual rethinking of the storage methodology.

Three storage methods were used:

Method 1: Deep cylindrical shaped pits with depths of 30 cm and diameter of 40–30 cm. The dried nuts were placed straight into a pit and capped with soil that contained pieces of flint and chert (Figures 2.1 and 2.2).

Method 2: Bowl shaped shallow pits with depths of 17–19 cm, diameter 50–60 cm at top. Nuts were placed straight into the pit and capped as above (Figure 2.3).

Method 3: The nuts were placed in a basket that sat on top of two logs and left a gap of approximately 5 cm between the basket and the sides of the pit. The pits had a depth of 40 cm and a diameter of approximately 55 cm at the top and about 35 cm at the base (Figure 2.4).

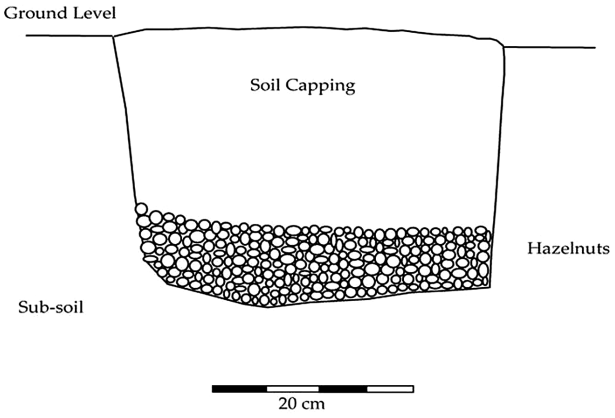

Figure 2.1. Schematic illustration of storage method 1.

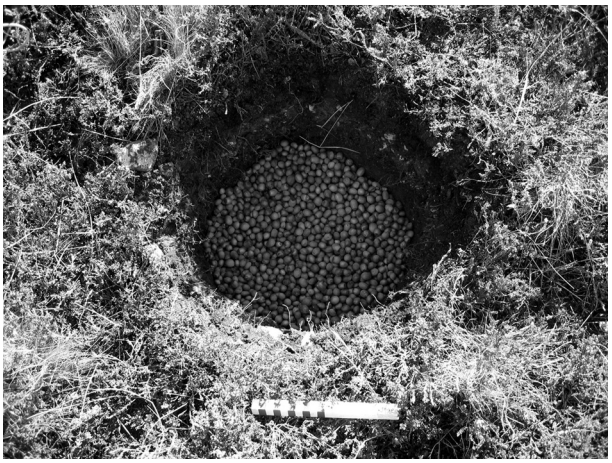

Figure 2.2. Method 1 – Pit 3.

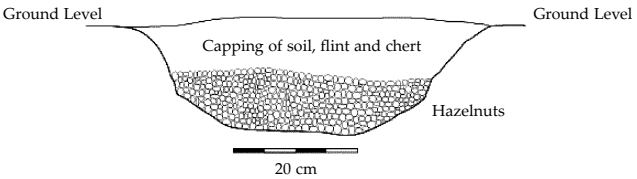

Figure 2.3. Schematic illustration of storage method 2.

Figure 2.4. Storage method 3: Acorns in basket.

The capping method consisted of a latticework of hazel twigs followed by a leaf layer and a final layer of soil, mixed with small pieces of flint and chert. This method does not create an anaerobic condition; in fact, it has quite the opposite effect, and allows air to circulate quite freely (Figures 2.5, 2.6, 2.7).

To determine the best storage duration, the nuts were stored for 20, 24, 26 and 32 weeks; taking the storage period beyond the time in which the nuts would normally sprout (Table 2.3). Once in the pits, the nuts were left undisturbed until it was time to retrieve them.

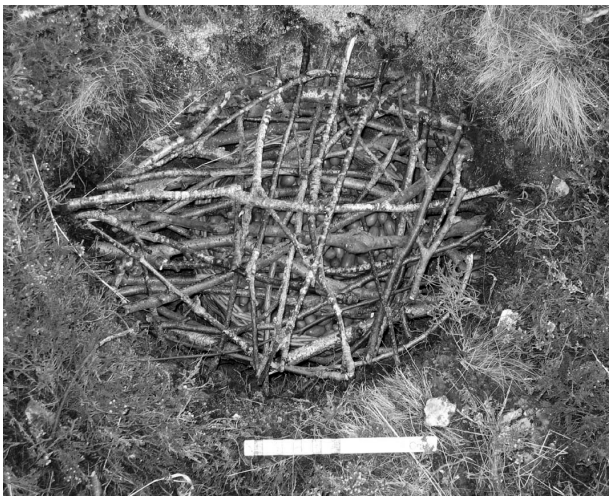

Figure 2.5. Storage method 3: Hazel twig lattice layer.

Figure 2.6. Storage method 3: Leaf layer.

Pit	Year	Duration	Nut Species	Method	Depth of Pit	Diameter	Quantity
1	2004–5	24 weeks	Hazelnuts	Method 3 Air dried, basket	40 cm	Top – 60 × 56 cm Base – 40 × 34 cm	3307 g
2	2004–5	24 weeks	Hazelnuts	Method 1 Air dried, straight into pit	29.5 cm	Top – 46 cm Base – 30 × 36 cm	3307 g
3	2004–5	32 weeks	Hazelnuts	Method 1 Air dried, straight into pit	29 cm	Top – 48–56 cm Base – 30 × 27 cm	3307 g
4	2004–5	24 weeks	Acorns	Method 3 Air dried, basket	40 cm	Top – 53 × 43 cm Base – 36 × 30.5 cm	3300 g
5	2004–5	24 weeks	Acorns	Method 1 Air dried, straight into pit	32.5 cm	Top – 44 × 41 cm Base – 28 × 25 cm	3300 g
6	2004–5	32 week	Acorns	Method 1 Air dried, straight into pit	33 cm	Top – 51 × 44 cm Base – 29 × 28 cm	3300 g
7	2005–6	20 weeks	Hazelnuts	Method 2 Roasted and air dried, straight into pit	19 cm	Top – 50 cm Base –20 cm	3800 g
8	2005–6	24 weeks	Hazelnuts	Method 2 Roasted and air dried, straight into pit	19 cm	Top – 60 cm Base – 25 cm	3800 g
9	2005–6	20 weeks	Hazelnuts	Method 2 Roasted and air dried, straight into pit	17.5	Top – 50 cm Base – 40 cm	3800 g
10	2005–6	20 weeks	Acorns	Method 2 Roasted, straight into pit	18 cm	Top – 45 cm Base – 20 cm	3215 g

Table 2.3. Storage methodology of all 12 storage pits, 2004–2006.

Pit	Year	Duration	Nut Species	Method	Depth of Pit	Diameter	Quantity
11	2005–6	26 weeks	Acorns	Method 2 Roasted, straight into pit	18 cm	Top – 44 cm Base – 20 cm	3215 g
12	2005–6	20 weeks	Acorns	Method 2 Roasted, straight into pit	20 cm	Top – 50cm Base – 10 cm	3215 g

Table 2.3 contd. Storage methodology of all 12 storage pits, 2004–2006.

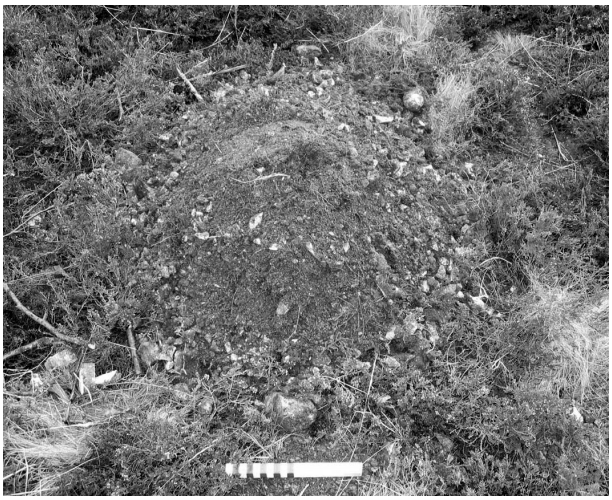

Figure 2.7. Storage method 3: Soil capping.

Recovery Procedure

The capping was carefully removed with a trowel to expose the top layer of nuts. Depending on the depth of the pits, the nuts were removed in 3–5 cm spits. Removing the nuts by hand made it possible to make a visual assessment of the condition of the nuts in each spit before placing the nuts into labelled bags.

Testing for Edibility

To determine the percentage of edible nuts, a random selection of 25% of the nuts from each spit were cracked open. Testing whether the individual nuts were edible relied on a subjective method of smell, taste and general appearance of the kernels/cotyledons;

nevertheless, it was very clear when they were rotten or edible. As acorns need other forms of processing to render them edible, edibility was based upon appearance, smell and texture.

Results

Hazelnuts 2004–5

Cracking open a random selection of 25% hazelnuts from each spit revealed that over 50% of the nuts were edible from Pit 1 (80%) and Pit 2 (84%) (Table 2.4). In these two pits, the edible and sprouted nuts looked very healthy and nutritious, indicating that although some had sprouted, a large percentage of the nuts were still edible. A greater proportion of the sprouted hazelnuts in Pit 3 had begun to decay; only those sprouted hazelnuts that were healthy in appearance were included as edible in this pit (52%).

In Pit 2, the nuts were very damp, but the pit itself did not retain any water, consequently, there were a larger percentage of edible hazelnuts. The greater quantity of sprouted nuts in Pit 2, compared to both Pits 1 and 3, might be linked with the temperature, reached within the pit. The compacted nature of Method 1 means that, in all likelihood, higher temperatures were created within the pit, in some respects, imitating the ideal conditions for hazelnut germination, and the wet conditions of Pit 3 and the basket in Pit 1 prevented the same degree of sprouting.

The results from Pit 3 also reflect the fact that this pit had an extended storage period (32 weeks) and the water retained in the pit. Unsurprisingly, Pit 3 had less than 50% edible nuts. Whether this is because of the wet conditions or the duration of storage is uncertain. In a previous series of storage experiments in 2001–2, the hazelnuts in a pit that retained water had a smaller percentage of edible nuts than those in a pit that stayed dry. A third pit also retained water, but the hazelnuts were in a basket, which helped to prevent them from decaying (Cunningham 2002, 66–67; 2005). The results from the 2001–2 and 2004–5 experiments suggest that the condition of the storage pits, rather than the duration of storage, caused the nuts to rot in Pit 3.

Acorn Storage 2004–5

There was more of an even distribution of edible, rotten and sprouted acorns in Pits 4 (31% edible) and 5 (29% edible), whereas Pit 6 (43% edible) had a very distinct difference between the two pits.

Pit 6 was damp and had a layer of mould, along with the majority of rotten and unsprouted acorns at the base of the pit, suggesting that the acorns in this pit were beginning to rot at the base. The largest quantity of sprouted acorns were located at the top of the pit suggesting that the depth of the pit, similar to Pit 5, but less than Pit 4, had a detrimental effect on the storage ability of acorns and the duration of storage enabled more acorns to sprout. However, Pit 6 also produced the largest number of edible and the least number of rotten acorns.

The results indicate that the acorns did not store as well as the hazelnuts using methods 1 and 2, as Pits 4 and 5 produced less than 50% edible acorns after 24 weeks.

Pit	Year	Duration	Nut Species	Method	Rotten	Edible
1	2004–5	24 weeks	Hazelnuts	Method 3 Air dried, basket	20%	**80%**
2	2004–5	24 weeks	Hazelnuts	Method 1 Air dried, straight into pit	16%	**84%**
3	2004–5	32 weeks	Hazelnuts	Method 1 Air dried, straight into pit	48%	**52%**
4	2004–5	24 weeks	Acorns	Method 3 Air dried, basket	69%	**31%**
5	2004–5	24 weeks	Acorns	Method 1 Air dried, straight into pit	71%	**29%**
6	2004–5	32 week	Acorns	Method 1 Air dried, straight into pit	57%	**43%**
7	2005–6	20 weeks	Hazelnuts	Method 2 Roasted and air dried, straight into pit	52%	**43%**
8	2005–6	26 weeks	Hazelnuts	Method 2 Roasted and air dried, straight into pit	48%	**51%**
9	2005–6	20 weeks	Hazelnuts	Method 2 Roasted and air dried, straight into pit	23%	**51%**
10	2005–6	20 weeks	Acorns	Method 2 Roasted, straight into pit	23%	**77%**
11	2005–6	26 weeks	Acorns	Method 2 Roasted, straight into pit	28%	**72%**
12	2005–6	20 weeks	Acorns	Method 2 Roasted, straight into pit	33%	**67%**

Table 2.4. Methodology and results from all 12 storage pits (excluding the blind nuts), 2004–2006.

Pit 6 stayed much drier and consequently the acorns fared better in this pit but still produced slightly less than 50% edible acorns, suggesting the conditions were still not right for the acorns.

Hazelnuts 2005–6

A mixture of roasted and unroasted hazelnuts was placed in the three pits in November 2005 and recovered between April and June 2006. The results indicate Pit 7 (stored for 20 weeks) produced the least number of edible and the largest numbers of rotten hazelnuts. As this pit was similar in size, but the duration of storage was less than for Pit 8 (stored for 26 weeks), the difference in the number of edible and rotten hazelnuts between Pits 7 and 8, is perhaps related to the condition of the nuts before storage. Pit 7 had a smaller quantity of roasted and larger number of air dried hazelnuts than Pit 8.

Additionally, Pit 8 contained only a few more sprouted hazelnuts than Pits 7 and 9 indicating that even though the roasting method did not fully roast all the hazelnuts, it dried them enough to prevent the majority from sprouting.

The results from the hazelnut storage experiment were in some respects surprising. The small percentage of edible nuts (43%, 51% and 51%) may mean that the storage conditions, a shallow, bowl-shape pit and damp conditions, prevented successful storage. Alternatively, we could suggest that roasting some of the nuts before storage does not increase hazelnut storage potential in a pit. The hazelnuts stored for the longest, in Pit 8, had the same percentage of edible nuts (51%) as Pit 9, suggesting that the poor results either are a direct result of the storage environment, or related to the condition of the nuts before storage or a combination of both. Nevertheless, if we take 50% edible to be the benchmark from which to measure success, we can see that the results have demonstrated that hazelnuts do store in shallow bowl-shaped pits, but the condition of the pit will affect their survivability.

Acorns 2005–6

The roasted acorns in Pits 10 and 12 stored successfully for 20 weeks, with both pits producing over 50% edible acorns (77% and 67% respectively). The number of edible acorns in Pit 11, 72%, was slightly less than those from Pit 10 but more than Pit 12.

The location, size, shape and depth of these pits were very similar to those used to store the hazelnuts, but as we can see, there is a stark difference between the storability of the two nut species. From the results, we could suggest that roasted hazelnuts do not store that much better than unroasted hazelnuts in the environment of the pit storage site. In contrast, roasted acorns store considerably better than unroasted ones.

Although during the 2004–5 experiment, large numbers of acorns and hazelnuts began to sprout, sprouting may have actually been desirable or at least not seen as a failure, as the sprouted nuts are still edible and the sprouted acorns could possibly be used to make beer as well as to eat. An additional variable is the species of acorn stored; *Q. petraea* (used in the 2004–5 experiment) naturally begins to sprout soon after falling and it is possible that this species is not suited to pit storage without some sort of processing to prevent sprouting and prolong storage. Whereas *Q. robur* does not naturally begin to sprout until the spring, indicating that they possibly have a longer storage life, whether dried or roasted, compared with *Q. petraea* (Mason 1992, 138; Jones 1959, 200).

Discussion

From the results, it is difficult to determine whether there is a limited time in which nuts can be stored because the pit storage environment does influence the duration of storage. For example, the hazelnuts stored in waterlogged conditions in a basket, stored for a longer duration than those in damp pits. In contrast, roasted acorns stored for longer in a damp pit than those in water-logged conditions. Additionally, the condition of the nuts prior to storage appears important. Roasted acorns stored much better than dried acorns and roasted hazelnuts, but as not as well as the dried hazelnuts. Therefore, the most important variables have to be the condition of the storage pit, the nut species and the condition of the nuts before storage, all of which needs further investigation.

The experiments demonstrate that the successful pit storage of hazelnuts and acorns requires an understanding of the climate and environment of the pit storage site and responding to these conditions in such a way that ensures a high success rate. For example, roasting acorns before storage will improve their ability to store and there is archaeological evidence of stored roasted acorns (Jørgenson 1977; Deforce *et al.* in press). The evidence of storage from Jomon sites in Japan may also be suggesting that storage methods differ due to both the type of nut species stored and the environment (Takahashi and Hosoya 2002; Habu 2004). However, this does not mean that the cultural or social factors did not play any role in the methods used, just environmental and climatic conditions do have to be considered.

We could also suggest that baskets may have been used as an insurance policy just in case pits retained water. In addition, using a basket would have made retrieval simpler as a person only has to lift the basket out of the pit, and a basket helps to keep the nuts clean whilst in storage, which would make any further processing easier. Thus, there are some practical reasons why nuts may have been stored in baskets (or other containers) within pits, beyond their ability to enhance their storage potential.

The experiments have demonstrated that hazelnuts and acorns will store in pits of various sizes. In addition, acorns and hazelnuts respond to their storage environment in different ways, consequently we cannot assume that there was a universal method by which to store both nut species and therefore we should not expect to see one in the archaeological record. Conducting the storage experiments enables alternative explanations for the presence of a variety of different shaped pits and nuts at both Neolithic and Mesolithic sites and that we can extrapolate that the storage of hazelnuts and acorns was possible throughout prehistory.

The problem is how to identify nut storage in the archaeological record. Throughout his paper, I have suggested that we have to look towards pits, and suitable resources produced in abundance. We also have to consider how storage would be an enabling practice, in other words what are the benefits of storing resources to both mobile and sedentary communities and why hunter-gatherers would practice storage when they can simply move from one resource to another. There are a number of reasons that we can put forward for storing resources; 1) for a period of known food shortage; 2) return to a location at a time when other foods maybe in short supply; 3) passing through an area on seasonal rounds, storage would make it unnecessary to burden oneself with

large quantities of food and 4) saving transporting to the main settlement/camp when produced in abundance.

If we incorporate these factors with the results from the experiments and the use of caches by more recent hunter-gatherers, we can suggest Mesolithic people left a cache of nuts along a route way for a time when passing through or returning to a camp at Lough Boora. Storing the nuts meant that there was a nutritious food resource waiting for a return visit to the site. Similarly, at Stasonaig storing nuts would ensure that they were available when required, for example, when passing by the island or when setting up a temporary camp to hunt or gather other resources. Storage would also have saved the problem of transporting a large quantity of nuts to a more permanent settlement elsewhere. The evidence of storage at the more permanent settlement of Mount Sandel, within one of the hut structures, implies that the nuts were either hidden or saved for a special occasion or when returning to settlement/camp after period of abandonment.

Thus, we can interpret the use of caches as an important hunter-gatherer risk buffering method to ensure resources where available during a time of known food shortage or to overcome the transportation problems caused by an abundant nut harvest. In the long term, the time and energy used to gather the nuts and prepare them for storage may have been worthwhile and made the nuts a high-ranking resource during a good mast year (Cunningham 2008). The location of the stored resources may also be linked with both individual and group identity, with rights to the stored resources restricted to only certain people (Cunningham 2008)

During the Neolithic, grain, as a dietary staple, was probably stored above ground where it was easily accessible (Rowley-Conwy 2000) or in large bell-shape pits, but nuts and other wild foods could have been stored in smaller pits and retrieved when needed. This type of storage suggests the use of nuts as an important supplement to the diet, particularly when the cereal crop was poor, and the continual exploitation of an old resource whilst adapting to a new one. In addition, as a means of bulking out a poor cereal harvest and ensuring the storage of grain for later replanting, nuts would have continued to have been included in the diet. Furthermore, nuts and other wild plants resources could also add flavour and contrast to the usual meals (Parker Pearson 2004, 13).

During the Bronze Age and Iron Age, small caches appear to have still been used and located within individual dwellings, whereas the larger pits and silos are located within large settlement used to store cereals and/or nuts. The difference in location is quite significant, as we can say that the acorns within a pottery vessel at Flögeln (Germany) and the storage practices at Fiavé (Italy) represent small, family level storage, perhaps hidden from the wider community. In contrast, we can suggest that the large pit storage of acorns at Evergem-Ralingen, Belgium as community level of storage (Jones and Rowley-Conwy 1984; de Ceununck 1991; Vencl 1996). Small-scale storage could also be associated with the storage of small amounts of ingredients that may have important usage such as for medicines or for alcoholic drinks rather than a major element of the diet.

Conclusion

By testing the storage of acorns and hazelnuts in small cylindrical and bowl-shaped pits, based on evidence from both Mesolithic and Neolithic sites in northern Europe, the current storage experiments move research of prehistoric storage practices in a new direction. No longer can we equate storage just with grain, large storage pits, granaries and sedentary communities; we have to now include the important role that storage played to both sedentary and mobile communities. The results from the experiments demonstrate that we can now suggest that the use of caches enabled mobility during the Mesolithic whilst providing additional, but important, supplements in the later prehistoric periods.

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3. Creating a History of Experimental Archaeology

Jodi Reeves Flores

Abstract

In order to assess the current state of experimental archaeology we must first study and analyse how it has been conducted in the past. The major initiation of experiment as a method for understanding past activities corresponds with the fruition of Western Society's first active attempts to understand ancient artefacts. By studying this early stage questions arise concerning what initiates such changes in how we view the past, as well as material artefacts. By moving on from this point in time, and placing major trends in the use of experiment in relation to developments within archaeology, we can begin to identify dominant relationships between practice, presentation and theoretical concepts. Such an endeavour creates a more knowledgeable environment within which to critically analyse why we use experimentation to study certain archaeological phenomenon. Perhaps most importantly it allows us to identify how experiment is viewed both as a methodology, and as a potential 'subfield' of archaeology. By placing experimental archaeology into a historical context, we can begin to critically evaluate the choices we make when conducting experiments, and it also creates the opportunity to consciously direct future developments within the 'experimental archaeology community'.

If one listens to a discussion concerning the status of experimental archaeology there is often a concern expressed regarding how it is presented, and how it is accepted. There is a perception that this has led to misunderstanding and possible rejection of data gathered through replicative experiment by the wider archaeological community. While this worry is not necessarily universal, it is prominent enough to be a major topic of discussion at several recent conferences and sessions on experimental archaeology: the Experimental Archaeology Conference at Exeter in 2007 (Cunningham *et al*. 2008), the last Experimental Archaeology Conference at Edinburgh in 2008, as well as this session on experimental archaeology at TAG in Southampton in 2008. This seems to be a major pre-occupation for this subfield of archaeology, and while it is not unusual (or at all negative) for the participants of a discipline to be critical of their methodologies

and approach to their subject, an impasse seems to have been reached regarding this discussion process in experimental archaeology.

Certain members often express frustration with the misunderstandings or indifference they receive from other 'types' of archaeologists. Those that attended the Experimental Archaeology Conference at Edinburgh will have experienced the frustration that many incorporating experimental elements into their research often feel. This was expressed in two papers at the conference: *More than the daily grind: experiments in grain processing techniques* presented by Merryn Dineley, as well as in a discussion of how the method is accepted by other disciplines in *Experience versus Experiment: differing disciplines' definitions leading to the answering of 'unanswerable' questions, a case-study using Roman dyeing* presented by Heather Hopkins. On the other hand, many practitioners have not experienced such difficulties (This is also discussed in a review of the conference by Roeland Paardekooper 2009, 61).

Why then do portions of experimental archaeologists view the method as excluded by wider archaeological circles? One possible answer is that this is, indeed the case. However, there appears to be stronger evidence that indicates that both historically and presently, well designed and presented experiments are just as accepted as any other form of archaeological investigation. A more productive conclusion may be that as a social group within academia, a portion of experimental archaeologists have developed a social memory that portrays the methodology as being outside accepted academic circles. Instead of taking a stance that analyses why some research is not as well received, a defensive wall seems to have been constructed. Not only does this shield individual participants from accepting criticism, but it also has the potential to turn experimental archaeology into a 'sub-discipline' rather than a methodology of archaeology. Such a development, I believe, is actually a hindrance in that it will limit the potential for other archaeologists to feel comfortable employing explicative experiments in their research, or to reach out to well educated experimental archaeologists (also see Cunningham, *et al.* 2008 for a discussion of this topic). Essentially, there is a danger of an experimental archaeology clique if we continue to view ourselves as separate from the wider archaeological community. To counteract this potential development, it is important to place experimental archaeology in its historical context. The aim of this paper is to evaluate claims concerning the acceptance of experiments by firstly creating a historical context for experiment, and secondly looking empirically at how experiments are accepted by the present archaeological community.

Contextual Framework

While this paper focuses primarily on textual evidence, it is part of a wider historically based, anthropological study of the nature of experiment in archaeology (Reeves Flores, forthcoming). Therefore, it draws from some basic concepts from both history and anthropology. This research assumes that present culture and institutions can be understood only within an historical context. This concept of historical awareness is based on three basic principles: difference, context and process (Tosh and Lang 2006, 6–23). The principle of difference recognises that time creates differences, or strangeness, between the present and the past, and the recognition of this gap primarily helps to

guard against any anachronistic tendencies that may be projected on the past. Context helps to place the strangeness of the past into its proper setting, so that it can be more properly understood. Closely linked to this is the idea of process. Process, like context, helps to create a framework for understanding the past, but in a temporal sense that helps to formulate the relationships between different events. Process is especially important when studying a specific field of history. No matter how detailed a study may be, if the historical events described are not placed in the proper context of wider processes, true understanding can never hope to be achieved.

Ethnology has also greatly influenced how this research has been conducted, and hence a brief discussion is necessary to establish the contextual framework of the study. As with any established methodology, ethnology's development has been affected by different theoretical schools. One such is positivism, which has also had a major influence on the social sciences, and continues to be a driving force in how archaeological experiments are developed and conducted (Outram 2008 states this clearly). According to positivism, scientific knowledge is based on observation of physical phenomena. Positivism goes as far as to claim that there is no way of obtaining metaphysical knowledge of a subject. Perhaps because of this, the processes through which theories are developed are excluded from scientific consideration (McGee and Warms 2004, 52, fn. 25; Hamersley and Atkinson, 2006, 6). Taking this into account, how can ethnology or historical research hope to achieve a scientific knowledge of culture?

This is why, instead of following the positivist line, many ethnographers embraced the concept of naturalism, which seeks to observe the world in its natural state, as opposed to the constructed experiments of positivism. The end goal here is more often a detailed description than a validated or invalidated hypothesis (Hamersley and Atkinson 2007, 7). While such descriptions themselves can supply important observational data, an emphasis on description tends to lead to the view that the processes being discussed as pure drama, upon which personal judgements can be placed. Such an approach does little to increase our understanding of social processes (Hoover and Donovan 2004, 6).

Perhaps a more profitable approach would be a reflexive realism (also see Hamersley and Atkinson 2006, 17). Indeed, there are statements about the world that on only slightly tainted by theory that they can be agreed upon by multiple people (Okasha 2002, 89). Ethnology is a method that can allow us to collect observations about the world that can then be double-checked and commented on by others. Indeed, this is a major goal of the social sciences, including history and archaeology, as a whole. In addition to this, such methods create a way for us to depict our understanding of the world in a way that is comprehensible to others (Hoover and Donovan 2004, 7, 10). Taking this into account, I know that in collecting data on the history of experimental archaeology, I needed to adopt a role as researcher that is both objective, but inclusive of the accounts I would be analysing. In this I am influenced by idea of culture histories, which seek to put history in a social and cultural context; the hermeneutical approach to studying texts; as well as heterophenomenology, or the "phenomenology *of another* that is not oneself", which is employed by the cognitive psychologist Daniel Dennett (2003, 2006). Heterophenomenology is especially useful, since it is basically an anthropological form of the scientific method—and in some ways alleviates the gap between positivism

and naturalism by taking into account an individual's report with all other available evidence that may have affected that report's development. Not only does this allow us to treat historical texts as data, but it also helps to reveal how the author saw the world without necessarily taking the wider accuracy of the text for granted.

While this methodological approach is rather flexible, it acts as a framework for approaching the key source of the bulk of the evidence: primary textual documents. Documents produced by the society being studied can provide a wealth of information. In the case of an historical study of experimental archaeology, these documents can consist of formal papers and essays, but also forms such as 'health and safety' and promotional and informational material from events such as conferences. For the present study we will be focusing on published academic texts, since the question being addressed is how the method was, and is, accepted by the archaeological community.

A History of Experimental Archaeology

Several key publications have helped to develop the way the history of experiment in archaeology is portrayed. The primary influence to date has been John Coles' 1979 publication *Experimental Archaeology*. While those such as Cole's history are primarily descriptive, others set out to address certain observations, such as Caroline Forrest's work on the role of amateurs in the historical development of archaeology (Forrest 2008). The following history is descriptive, but also places experiment into its important role as part of the development of archaeology as a whole. Such a history moves towards addressing the idea of social memory that has developed among experimental archaeologists as a social group. Social memory refers to how a group seeks to justify or explain present circumstances. While based on historical events, social memory is different from history because it is often modified (consciously or unconsciously) to meet group needs (Tosh and Lang 2006, 12–3). I believe that certain portions of the experimental archaeology community have developed a social memory that helps to explain feelings of exclusions from the wider archaeological community.

By outlining a history of experimental archaeology that shows that the method has been accepted through most of the development of archaeology, I hope to address this false sense of history. However, this does not advocate complete denial of these feelings of exclusion. What needs to be done is establish an objective baseline of the history of experimental archaeology from which to address the feelings expressed by members of the community. Hopefully this will help to identify the real reason as to why some feel excluded, and also to establish if there is empirical evidence for such exclusion.

The following descriptive history is divided into general developmental categories, which are based on perceived and actual trends regarding the development of archaeology and the role of experiment in archaeology (see also Coles 1979). The first period is one in which there is no real presence of archaeology in general, however the social and ideological groundwork is laid for studying the past though material remains. The second period, from the early eighteenth century to the middle of the nineteenth century documents the formulative period of archaeology as a discipline. This period blends into the period after it, in which archaeology becomes a professional field of

study. It is hard to mark a specific date in which this happens, but this turn in events is traditionally allotted to the middle of the nineteenth century, and for the present moment we will follow this inclination. Some experimentation is practiced during this period, although it seems primarily linked with prehistory. However it is not until after the 1850s that experimental archaeology really comes into its own, with the rest of archaeology. This continues until the early twentieth century, when the development of culture historical archaeology, as well as other events, affect the role of experiment and the progress of archaeology as a whole. It is not until after the Second World War that replicative experiments again become prominent. The increase in explicit theory leads to the discussion and formulation of 'experimental archaeology' in the 1960s. The development of post processualism in the 1980s eventually leads to the intense debate we are in today about the role of experimental archaeology in exploring the past, and its place in the wider archaeological community.

Laying the groundwork: studying the past before AD 1700

Before the eighteenth century, those that studied the past and history focused primarily on written texts, and there was almost no inclination towards using material culture as a resource for information about the past. However, changing social situations throughout Europe would help set the stage for the development of antiquarianism and archaeology in the eighteenth and nineteenth centuries. Economic, political and social developments led to an increase in nationalism throughout many parts of western and northern Europe. Many states began to look to the past to justify their existence and promote a sense of pride. This, combined with the realisation that humans could invoke drastic change in the world, led to an increase interested in past events and processes (Trigger 2006, 78–9).

One such major development was the creation of mercantile city-states such as Florence. These newly emerging cities needed to justify both their existence, and independence. A major source of inspiration for creating identity was found in Classical remains (Trigger, 2006, 52–4). Supported by these developments, the systematic study of archaeological remains truly begins to develop in the fifth and sixteenth centuries. In the fifteenth century, the term antiquarian begins to be used to refer to people who study archaeological remains, and by the sixteenth century professional antiquarian positions are created.

Another important change during this period was the Protestant Reformation, which affected much of Northern Europe and Britain. The Protestant Reform, as it was experienced in England, also had a great affect on how people interacted with ancient material remains. When Henry VIII (1509–1547) declared the destruction of the catholic monasteries, landscapes that people had known for generations were altered. Many also recognised that a great amount of information, both in the form of text and materials, was being lost in the process. In England, as well as in Northern Europe, the interest in local past was aroused primarily in the raising middle class, who far from being overly wealthy, still had enough leisure time to devote to exploring and describing their local surroundings, as well as the antiquarian studies of those areas (Trigger 2006, 84–5).

However, one of the widest reaching changes was the development of the

Enlightenment in the seventeenth century, which refers to the major developments in philosophy and culture during this time. One primary component of the Enlightenment was the development of a positive view of progress and technological advancement. This was combined with a more hopeful view of humanity's, and the individual's, place in the world. These ideas would be essential in fostering a worldview in which the interest in technological advancement would flourish. However, what was need for experimental archaeology to flourish was an interest in *past* technological achievements, as opposed to present or future advancements. This was partially supplied by nationalism, which continued to be a primary movement in the seventeenth century. Countries, as well as the middle class, turned more and more to the past for evidence to support their identity.

The increased navigation of the world that fostered the mercantilism that shaped Europe during this period also meant that there were clear material changes that helped to create an interest in primitive technology, primarily the interaction with stone tools from the Americas that was proliferated as European countries increased contact and trade with this part of the world. The increase in knowledge of American cultures that used stone tools led to the realisation that similar stone objects being found all over Europe were manmade (Trigger 2006, 82, as well as Coles 1979, 3–5).

Developing the field: archaeology and experiment from the eighteenth to the early nineteenth century

However, not everyone was content with the accelerated changes that had begun to take place since the end of the medieval period, and this led to an increase in conservative thought and yearning for the past. Romanticism, coupled with the conservative backlash against cultural-evolutionary thought did have a far greater influence of the development of antiquarianism of this period than did cultural-evolution. However there is also a dramatic increase in archaeological excavation during this time, and there is yet to be any real proof that there was a decline in how this evidence was interpreted (Trigger 2006, 110–3). However, the shift to a romantic view of the past and its emphasis on excavation, coupled with advances in approach and interpretation beforehand in the early part of the seventeenth century helped support a rise in excavation.

It is perhaps this romantic view of the past, coupled with increased interest in material remains, which encouraged the physical exploration through experimentation and replication of past remains and processes. Early experiment during this era is perhaps typified by the work carried out on bronze instruments that were uncovered throughout the period in both Britain and Denmark. These musical instruments began to be uncovered in Ireland as early as 1698. The first one found was also tested to see if it could be made to play any musical notes. The sound was considered low and dull (Coles 1979, 13). A contemporaneous bronze instrument, called a lurer, was found in Denmark in moorland at Brudevaelte, Zealand in 1797. It is thought that Christian Jurgensen Thomsen, creator of the Three Age System, was the first to experiment with the *lurer*. Later, J. J. A Worsaae also experimented with the instruments, and in 1843 recorded that the horns could be played, but emitted only dull sounds (Coles 1979, 13). The *lurer* and other bronze instruments continued to be a focal point for experiment through out the nineteenth and early twentieth century.

While interaction with stone tools from other cultures let to the realisation that similar artefacts found in Europe were potentially manmade, finds such as the bronze musical instruments indicated that different materials may have been in use at different points in time by European populations. The study of such tools eventually developed into prehistoric archaeology in Europe. According to Trigger (2006) this development took place in two waves, the first of which began in Denmark in 1816, and the second that developed fifty years later in England and France. One of the primary reasons as to why it took so long for prehistory to develop was that in the early 1800s, antiquarians and archaeologists simply did not see the necessity of having a field of prehistory, because there was no concept of there being a deep human past (Rowley-Conwy 2007, 3). While it is true that by the 1840s there was an established concept of deep geological timescale, this had not yet been applied to the human past (Rowley-Conwy 2007, 5). Replicative experiments and their application in studying archaeological remains are deeply tied to the development of prehistory.

This is especially made clear in the work of Scandinavian archaeologists who developed an evolutionary approach to studying culture (Trigger 2006, 164). One such prehistorian was Sven Nilsson, who was a Professor of Zoology at the University of Lund. As with Thompson, Nilsson was interested in cultural evolution, but focused primarily on the development of subsistence economies. As a contemporary of Thompson, he actively employed ethnographic evidence as well as replicative experiments in developing his study of the stone age, which was published in four parts between 1838 and 1843 (Trigger 2006, 129–30).

Not only were Scandinavian prehistorians using experiments to study technologies, they were also integrating it into wider archaeological projects. In 1848 the Royal Danish Academy of Sciences created an interdisciplinary commission to study shell middens. One of the heads of the commission was J. J. A. Worsaae. The reports published on the research undertaken by the commission mentioned taphonomic experiments which had been undertaken in order to explain why more middle parts of the long bones of birds were present in comparison to other parts of their skeletons (Trigger 2006, 133).

Towards the end of this period, prehistory begins to be properly developed in Britain and France in the form of Palaeolithic archaeology, which was modelled primarily on the natural sciences (Trigger 2006, 164). However it appears that before this point, the primary experiments in Britain with prehistoric materials focused primarily on the bronze horns mentioned previously. Indeed, such bronze musical instruments continued to be found in Britain throughout the eighteenth and twentieth century (see Coles 1979, 13 for a list of some). However, it is not until later that archaeologists truly begin to exploit replicative experiments in exploring the origins of early humans.

It is also during the end of this period, between 1780 and 1860, that archaeology in North America seems to progress through a phase similar to the antiquarian phase that England and Scandinavia passed through between the sixteenth and nineteenth century (Trigger 2006, 161). While studies in Europe continued to focus primarily on Classical and Near Eastern studies, archaeology in North America saw an increase in the interest in indigenous archaeology. However, it was heavily dominated by racist views of indigenous populations that helped to support the extermination of populations (Trigger 2006, 158–9). Despite these dominate ideological influences on

the interpretation of archaeological material from North America, it can be said that in general, the level of research and the publication of that research continued to become more systematic during this period. Societies and groups, such as the American Antiquarian Society (begun in 1812 by Isaiah Thomas), created a forum in which the budding north American archaeology community could develop (Trigger 2006, 161). As part of this development, and investigation into the past of the native populations of North America, experiment would become an invaluable tool for North American archaeologists in the coming century.

While there were definite increased interests in other periods and geographic regions, there was still a major emphasis on studying classical and near eastern archaeology during the eighteenth century. In present literature there does not seem to be any indication that there was much of an application of experiment to materials being studied by classical archaeologists of the period. This may be due to the fact that these disciplines continued to be primarily based on the study of art and of text, as opposed to technology.

Mid-nineteenth century – early twentieth century: the establishment of archaeology, and the development of experiment

Traditionally archaeologists view their field as being established during the middle of the nineteenth century. On the world stage, industry was increasing dramatically across Europe, which was coupled with a renewed belief in the positive affects of progress, and increased interest in the idea of cultural evolution (Trigger 2006, 145–6). Material progress in terms of building and road construction also meant that archaeological materials were being uncovered at an impressive rate (Trigger 2006, 215).

Archaeologists themselves continued to be interested in creating chronologies for archaeological materials and periods, and Palaeolithic archaeology continued to develop. The discipline became more systematic and professional. Until the 1880s, archaeologists continued to focus on cultural evolution (Trigger 2006, 207, 215). However, as interested shifted to explaining the movements of human populations through diffusion, archaeologies began to focus more on culture histories (Trigger 2006, Chapter 6). While this switch from evolutionary archaeology to culture history may have eventually led to a decline in experiment in archaeology in the early 20th century, the methodology develops and maintains a state of popularity for most of this period.

The use of replicative experiment in prehistoric studies continues to be the norm, although it is worth mentioning that W. M. Flinders Petrie, prominent Egyptologist, promoted the study of technological processes (Coles 1979, 19). In North America, experiment develops as a methodology with close links to ethnography, and the study of historical populations. Prehistoric studies in Europe, including Palaeolithic archaeology, also begin to use replicative experiments to study ancient materials, particularly chipped stone tools. There is a wealth of experiment that takes place during this period, as can be seen in Coles (1979) discussion of the subject. Instead of surveying all of the experiments that took place, I would like to focus on those conducted by Frank Cushing and J. D. McGuire in the United States and Hezzledine Warren's experiments with eoliths.

The achievements of prehistoric native populations of North America continued to be downplayed, or outright dismissed, during the nineteenth and twentieth century.

However some archaeologists used historic ethnographic data and early European records in studying the production and use of prehistoric artefacts (one such example is Cushing 1894). While many studies focused on different stone technologies, J. D. McGuire and Frank Cushing experimented in replicating copper disks that were associated with the Hopewell Culture of Ohio. Using techniques that would have been available to indigenous populations, McGuire and Cushing were able to show that the disks were not an European technology. Cushing especially took exception to this idea and was very vocal on the point that indigenous people would have had the skill and expertise to produce such works of art (Cushing 1894; Coles 1979, 23–5). His work on worked copper not only integrated experiment into the study of prehistoric material remains, it also used those experiments to directly question deeply held ideologies regarding the ingenuity of Native Americans.

Meanwhile in Europe, experiment was playing an important part in the debate over eoliths, chipped stone objects thought to be early prehistoric tools. It was thought that producers of early stone tools would not have followed as uniform procedures of manufacture as later peoples. Therefore any stone that had regular chipping and a relatively uniform shape was of possible human manufacture. Hezzledine Warren developed and carried out a methodology for testing whether normal geological factors could yield results similar to what was being seen in eoliths. Warren published extensively on the topic from the early part of the twentieth century onwards (1905: *On the Origin of Eoliths*). The results and conclusions of his experiments were often criticised, as can been seen in the dialogue that took place between Barnes and Moir and Warren in the 1923 volume of *Man* (see Barnes and Moir 1923 and Warren 1923).

While much discussion is given as to the mechanics of the experiments, and the interpretations and presentation of results, the actual use of actualistic experiments appears to be a given. This is most likely that because by this time, experiment was considered a legitimate archaeological method (Coles 1979, 26–7). However, the next two decades would see a dramatic decrease in experiment, until the method finally re-emerges as a full-fledged experimental archaeology.

Early twentieth century to mid-twentieth century: the gap years

According to Coles, Warren's work with eoliths marks the end of major projects employing experimental and replicative techniques in studying the past. While English archaeologists did some work in the 20s and 30s, the use of experiment as a methodology would not blossom again until Scandinavian archaeologists once again took it up in the 40s and 50s (Coles 1979, 29). Indeed, there are several developments that take place during the early and middle of the twentieth century that no doubt affected archaeology and the use of experiment to explore the past. Most notably, there were two world wars, and economic depression. However, in their own ways these events actually increased the amount of archaeological work being done. Two such examples would be the work projects in the United States that were developed to increase employment, and the support given to prehistoric archaeology under the Nazi regime in Germany.

Other developments, however, specifically affected the ability or desire to conduct replicative experiments. For the past two hundred years, replicative experiments had

been closely tied to ethnology and information that could be gathered through observing indigenous populations. However, many people and their traditional ways of life had been irreparably changed by Western migration (Coles 1979, 27). This potentially affected experiment in two ways: firstly the ability to observe tools in use or to gather information on their use was greatly reduced, secondly anthropologists of the period no doubt recognised the damage being done, and shifted more towards recording information on cultural and social practices before they became extinct.

Another factor came from within the archaeological discipline itself. During the early part of the twentieth century, many archaeologists throughout Europe began to shift from cultural evolutionary archaeologies to those that focused on culture history. This movement was greatly aided by the work of V. Gordon Childe, such as his 1925 *The Dawn of European Civilisation*. While the culture history movement did much to increase the explicit dialog regarding theoretical approaches and interpretation of data, the focus of archaeology went from understanding technological changes to mapping changes in population and cultures. This shift in preoccupation served to weaken ties between archaeology and ethnology, even in North American were many archaeologists had relied heavily on ethnographic information before this period (Trigger 2006, 288–9).

Post-war and processual archaeology: the emergence of an 'experimental archaeology'

Archaeology after the Second World War was different from previous eras in that there was much more of an emphasis on thoroughly and objectively recording archaeological data, and in the fact that archaeologists had begun to acknowledge that there were limits to what archaeological data could tell us about that past (Coles 1979, 30–1). Not only did technological advancements such as the introduction of C-14 dating increase the scientific aspect of archaeology, computers increasingly made it possible to store and analyse large amounts of archaeological data. This more 'scientific' approach to studying the past is best exemplified by the New Archaeology movement begun by Lewis Binford in the 1960s with publications such as *Archaeology as Anthropology* (1962), which also happens to reference Cushing's work with copper. Perhaps most importantly, Binford's work re-established the importance of using ethnographic evidence in archaeological research that had been diminished during the period of culture historical archaeology.

However, before New Archaeology reinvigorated the use of experiment in the United States and Britain, Scandinavian archaeologists had already revived the tradition in the 1940s. Perhaps one of the most well known experiments continues to be Thor Heyerdahl's voyage on the Kon-Tiki, which he sailed from Peru to Polynesia in an attempt to show that people could have migrated from South American westward (Heyerdahl 1950). Another major development, which was pursued by Scandinavian experimentalists, was the foundation of the Historical-Archaeological Experimental Centre at Lejre in the 1960s (http://www.sagnlandet.dk/). During the same period Peter Reynolds established an experimental centre at Butser Hill in England (http://www.butser.org.uk/). While the sites differ in their approach and presentation, both have had a major influence in the development of experimental archaeology since their inception. The general trend appears to be that experiment is still commonly used to

study prehistorical or non-historical cultures, and is still limited to its application to historical periods, especially in the realms of classical archaeology.

It is also during the 1960s that there is an increase in the literature specifically discussing the use of experiment in archaeology. Forrest notes that Robert Ascher's article *Experimental Archaeology* (1961) appears to be the first use of the term, and to date I have yet to find an earlier incident (Forrest 2008). This is an extremely important trend because it marks the point from which experiment moves from a method that is commonly integrated, to an explicit methodological framework. Shortly after this there is a proliferation of articles on the topic. Many, such as Asher (1961) and Coles (1973, 1979), attempt to define the methodology as well as give examples of work that had been undertaken.

Thanks to the new ideological trends established by New Archaeology, and the pioneering works of experimental archaeologists, the methodology was not only once again being applied in research, but also actively being discussed. However, towards the end of this period, Saradyar and Shimada claimed that "the potential contributions which imitative experiments can make to archaeology are far greater in scope, complexity, and overall 'value' than is commonly realized (Saradyar and Shimada 1973, 344)" and rallied for a complex view of the role of experimental archaeology and a re-evaluation of the possible methods of application (Saradyar and Shimada 1973, 349). They also hypothesised that as archaeology and the questions archaeologists developed continued to develop and become more complex, so must the way in which we approached experimental archaeology if it was to maintain a useful methodology (Saradyar and Shimada 1973, 349).

Present Practice of Experimental Archaeology

1980s to today: experiment in practice

The development of middle-range theory (Binford 1981) helped to further establish a framework from which the theoretical aspects of experimental archaeology could progress. However, the emergence of the post-processual movement during the 1980s questioned the scientific, objectivist stance of New Archaeology. By doing so, postprocessual archaeologists opened up the way in which archaeological materials could be approached and interpreted. While one of the main tenets of post-processual archaeologies is to explore the different interpretation of the past, there does not appear to be a specific movement to embrace experimental archaeology on a grand scale as there was with New Archaeology and middle-range theory. However, post-processualism has called into question the role of archaeology and the archaeologist, as well as advocating the need for more critical evaluations of our work. This has, no doubt, influenced the present state of the *experimental archaeology community*. During these past three decades, the use of experimental archaeology has continued to proliferate. It is now taught at the university level, as well as being the focus of an entire Master of Arts degree at the University of Exeter since 2000.

This brings us to the recent discussion on how experimental archaeology is viewed and presented. Despite a hiatus in the middle of the twentieth century, replicative

experiments have always been a method that is well integrated into archaeology as a whole, especially in relation to topics that are considered part of the realm of prehistory. However, as the literature referred to at the beginning of this paper indicates, there appears to be the assumption that experiment is not well accepted at present. In order to evaluate these sentiments, we will now look at publishing records from three well known archaeological journals, and survey recently published works on the topic of experimental archaeology.

Publishing experiments in journals

Analysing the publications of archaeological experiments is important for several reasons, but primarily because it is the most public and prominent way to show that the experiment is considered acceptable by the standards of the audience to which the publication is meant to appeal. Here I will be looking specifically at the rates of publication for three major, peer-reviewed archaeological journals: *Antiquity*, *American Antiquity* and the *Journal of Archaeological Science*. These three journals were chosen for the fact that they are relatively broad in topic and accessible, as well as viewed positively, by most of the western archaeological community. Also important is that not only are issues of the journals often subscribed to by institutions and individuals, they are also available online for subscription, or accessible through prominent databases, such as *JSTOR* or *EBSCO* which are available to many academic archaeologists. Not only does this issue of accessibility make researching these journals easier, it means that those interested in experiment, or in research that contains experiments, can quickly find such articles. Therefore, not only are the articles being published, but also they are active parts of the continuation of the methodology.

The first two journals, *Antiquity* and *American Antiquity*, are quarterly publications that publish on a wide range of archaeological topics. *Antiquity*, which is not associated with a major institution, has published in the United Kingdom for over 75 years, during which time it has actively fostered wide range in terms of submission, types of articles and readership since its inception (DeMarrais 2002, 1089; Kohler 2002, 1121). *American Antiquity*, as the title implies, is published in North America by the Society for American Archaeology (SAA). While the dominant content covers American archaeology, other topics are often included, and the journal has a wide readership base, which includes both professional archaeologists and amateurs that are interested in the discipline (Kohler 2002, 1121–2). The *Journal of Archaeological Science* is published monthly, and has been printed since the 1970s. While articles cover a wide variety of subjects, the primary theme that links the articles included is that the research has some sort of 'scientific' basis. This can include the application of methods such as analysing isotopes of DNA, testing the practicality of new technologies in archaeological research, as well as lab based, and replicative experiments.

Methodology

Before surveying the journals for replicative experiments, I needed a definition of what type of experiments would be included in the calculations. When looking at the history of experiment in archaeology, I allowed some leeway, and included research, which attempted to replicate or test artefacts and processes with the intent of gaining

information about the past. However, a more definitive definition was needed for the collection of modern publication rates. It is also more applicable to modern material because of the contemporary actions of discussion and definition that have taken place in the last 30 years. Therefore I decided to define replicative experiment as:

> 1) the process of replicating past material culture, conditions, and/or processes in order to address a hypothesis, as well as 2) the use of products resulting from the replication of past material culture, conditions and/or processes.

This definition was applied to all aspects of the articles under study. Therefore, the research being presented was not entirely based around a replicative experiment, but instead employed replicative elements to address and archaeological question, the article as a whole was included. In terms of understanding how archaeologists accept experiment, these articles are very important because they illustrate that the methodology can be, and is, often integrated into wider research schemes.

In order to quickly assess the recent rates of publication of replicative experiments in each of the three journals, all the issues for the years 2002, 2005 and 2008 were analysed. Future work will continue this process (1999, 1996, etc.). The methodology was as follows:

- All issues of the respective journals were collected for the chosen year.
- Each abstract for articles that covered research was read and evaluated for possible experimental content.
- Articles that were deemed to possibly contain experimental material based on the previous step were read over.
- All articles that included experimental elements were included in the final calculations.

While this may seem straight forward, several issues developed over the course of trying to identify which articles would be included in the calculations – especially when it came to analysing articles from *Antiquity* and *American Antiquity.* Another problem was delineating between articles that contain experimental elements and those that contain *replicative experiments*. While the application of the experimental method itself is very important, the topic we are most interested in is the acceptance of replicative experiments. This no doubt has some affect on the final calculations, especially when looking at the results from the *Journal of Archaeological Science.*

Analysis and context

Before discussing the results, I would like to mention certain variables that may have affected the final outcome. First, note that in 2002, *Antiquity* celebrated its seventy-fifth anniversary, and there were a larger number of articles than normally expected. The vast majority focused on the history of archaeology in general, and many discussed aspects of the history of *Antiquity*. Likewise, in 2007 *JAS* increased its page budget, allowing for the acceptance of more articles (Rehren 2008).

While this is more circumstantial, other factors had to deal with the nature of the research itself, primarily the process of identifying experiments as replicative. The primary difficultly was distinguishing between laboratory experiments,

ethnoarchaeological observations, and replicative experiments. For example, does the creation of experimental cut marks on bone in order to test a new method of three-dimensional analysis count as a replicative experiment? In determining whether something was ethnoarchaeology or experimental archaeology, it was based on whether the researchers participated in the activity either by actively taking part in the processes being studied, or by setting up an actual experimental process including the materials being used. Research that was based on pure observation was not included in the data calculations (see Table 3.1).

This data shows that the use of experiment in archaeological research is relatively common and accepted by the larger archaeological community. However, it does appear that replicative experiments are much more likely to be included in research that is considered more *scientifically* oriented. This may explain the larger percentage of replicative experiments included in *JAS*. However, they are not completely limited to this genre, as inclusion in *Antiquity* and *American Antiquity* show. It is also prudent to point out that since replicative experiments normally follow a procedure that includes scientific methodology, it would make since that there is a large number of such articles in a science oriented journal. Experiment seems to be relatively well accepted by the archaeological community in terms of publishing in general journals. However it has also been major topics in other publications. For example, the World Archaeology's 2008 volume focused exclusively on experimental archaeology (Outram 2008). While the data collated thus far is only for the last several years, there is significant evidence that experiment is considered a viable methodology where academic archaeology is concerned.

Year	Journal	Volume	Number of articles	Percentage (%) containing replicative experiments
2002	Antiquity	76	91	0
	American Antiquity	67	39	7.69
	Journal of Archaeological Science	29	118	8.47
2005	Antiquity	79	54	0
	American Antiquity	70	32	6.25
	Journal of Archaeological Science	32	154	12.99
2008	Antiquity	82	63	3.17
	American Antiquity	73	33	3.03
	Journal of Archaeological Science	35	291	8.25

Table 3.1. Percentage of articles containing replicative experiments.

In terms of experimental archaeologists publishing for their own *community*, the proceedings from the 2007 Experimental Conference in Exeter, UK were published in 2008 (see Cunningham *et al.* 2008). There are also the journal *euroREA*, which is a yearly European publication that includes articles on experimental archaeology, as well as other replicative projects, and reproductions. Several conferences have also been held, including the Experimental Archaeology Workshop in 2006 at UCL, the Experimental Archaeology Conference held in Exeter in 2007 and in Edinburgh in 2008, with another conference planned for 2009. There have also been sessions on experimental archaeology at major conferences, including at the Society for American Archaeology meeting in 2008 in Vancouver, British Columbia, and at the Theoretical Archaeology Group in Southampton in 2008, with another session planned for the 2009 Theoretical Archaeology Group at Durham.

Conclusion

A survey of the historical development of experimental archaeology and the present rates of publication on the topic strongly indicate that replicative experiment is not only an accepted methodology, but utilised widely in the literature researched. Therefore, there is no solid justification for experimental archaeologists as a group to adopt a feeling of exclusion as a part of the community identity. By accepting that, on the whole, experiment is well received we can begin to systematically evaluate areas in which it is not. An integral part of this is to maintain that experimental archaeology is a *methodology*, not a subfield such as wetland archaeology. While it is important to create a dialogue between those that practice experimental archaeology, there must be a critical evaluation of attempts to create an insular group or community.

Once this ideological stance is adopted, we can begin to analyze why certain experiments are not accepted within the subfields that they are presented. One example is Hopkins' (2008b) discussion of how difficult it was for her methodology to be accepted within the field of classical archaeology. Historically, replicative experiments have not been an integral part in studying text-based societies. By experiment with dye-working facilities, Hopkins was undertaking a project that does not have an established basis in the literature of that chosen academic field, as would an experiment with lithics or pottery might. An initial step in addressing this issue of exclusion would be to reach out to classical archaeologists and, as Hopkins has done, illustrate how replicative experiments can not only complement archaeology and text based studies, but supply data not normally accessible through these means (Hopkins 2008a). As with any research, the way in which experimental archaeology is presented is of major importance, and while this subject as been discussed previously, such as in the 2005 edition of euroREA, it is worth mentioning once more. This is perhaps why non-academic experimental archaeologists may feel excluded. It is not so much an issue of subject or content, but rather of presentation, as well as possible biases, not against experimental archaeology, but against amateurs in general. However, there is now a widely available amount of literature addressing this issue, and it is possible to overcome such a hurdle for the most part (Outram 2005; Mathieu 2005; Schmidt 2005). Therefore, while some practitioners way feel marginalised, it is primarily because of

the tendency to view, and therefore present, experimental archaeology as a sub-field rather than a method. If it is instead viewed as a method that complements research in other aspects of archaeology, we can begin to actively integrate it into wider research practices and continue to develop experimental archaeology into an indispensable archaeological methodology.

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4. Breaking the Sound Barrier – New Directions for Complexity, Transformation and Reconstructive Practice in Experimental Neolithic Archaeoacoustics

Claire Marshall

Abstract

When considering recent interpretive developments in the Archaeology of the Neolithic, it is apparent that visual cues in disseminating the archaeological 'record' are dominant over the primary sensory experiences we as human beings rely upon every day. In constructing a view of our prehistory we are confronted with fragments of a culture seemingly alien to our own and are left to erect biographies based upon inference. In considering the importance of radical experimental archaeology as emergent novelty we are able to include the notion of 'approximation' as a valid path to understanding our ancestral prehistory.

This paper will consider research in sound archaeology and its implications for how we view the British Neolithic through the controversial paradigm of emergent novelty. Through the implementation of a pilot project combining Neolithic archaeoacoustics and reconstructive organology (sounding devices from animal remains), we are able to gain an understanding of social dynamics and complexity where transformation, and the skills to achieve such transformation, play a central role, thus addressing in new ways our problematic dependency upon traditional culture/nature dichotomies.

Situating Theory with Practice

Few areas of research have been more central to what previously has been considered as parting of the ways of new and post-processual archaeologies. Many of the presuppositions mentioned in the previous chapter have impeded strongly on the adoption of fresh approaches to our data. The situation is changing, not least of all through the increasing attention that is being given to the relevance for new directions of research on the Neolithic of insights drawn from Tim Ingold's work (for instance, Thomas eds 2000) into the novel and unpredictable ways in which experience is constructed

at the contingent level. As Koerner (2008, 2009) has iterated, the ethical implications for Latours quote for the common world as 'not something we can come to recognise' (Latour 1993) have far reaching implications for the ways in which experimental archaeology can be conducted. We are bound by notions of the experiment proving a predetermined hypothesis (Binford 1968), rather than opening up new ways in which we can view both our past archaeology and our present reliance on the deterministic nature of hypothesis testing. My purpose in this discussion is to problematize some of the theoretical arguments relating to predetermination by considering methods by which anthropology and molecular science can be touched upon as analogy to skills that are manifest in the archaeological record. This in turn brings to bear the importance of ecological engagements in the past and provides an optimistic alternative for the inclusion of creative techniques in the way experimental archaeology is conducted. This discussion will consider both the theoretical underpinnings of spontaneous creativity in nature (or Emergent Novelty as used by biological anthropologists) and situating that creativity into case studies of experimental archaeology from existing archaeological data. My case studies will look at current research into material and organic remains at Neolithic contexts in Britain and my own research into sound/acoustic and sensory archaeology at sites of a contemporary nature.

The Theory of Ingold and Prigogine: Indeterminancy in Nature and Skills

The work of anthropologist Tim Ingold is particularly pertinent for our understanding of how the culture/nature dichotomies are in fact hindering the advancement of our knowledge of the archaeological past. Ingold's approach has challenged some of the presuppositions of science as laid bare by Koerners critique of determinism in science (Koerner 2009). Particular attention is given here to Tim Ingold's work in addressing major objections to experimental archaeology raised by post-processualists (Binford 1969), and addressing problems with the nature/culture dichotomy in general by challenging the presuppositions upon which these deterministic visions of the world hinge. Ingold's work can be drawn from parallels in other disciplines such as that of molecular physics. Physicist and Nobel Laureate in Physical Thermodynamics Ilya Prigogine challenged the assumptions of these dichotomies by exposing the importance of creativity and improvisation at the molecular level in the seemingly random ways atomic structures came into being thus situating the phenomena of emergent novelty (Prigogine 1997). He advocated that irreversibility in nature (or the phenomena by which self organisation in molecular and cellular formation cannot be undone) was normal, contingent and essential for the ways in which these structures interact with their environment (*ibid.*). Prigogine understood that to overcome determinism in nature, the very basic atoms themselves form complex social relationships so that they may cooperate in building something new, emergent and creative, and that conditions in nature are not subject to all factors moving towards an equilibrium as is suggested by the foundational laws of Newtonian Mechanics (*ibid.*). Prigogine's work formed a potent critique of the neo-Darwinian biology associated with linear forms of development

centred on complicated descriptions of physical and chemical processes of the organism (Koerner 2007). It suggested that instead of us considering the split of social and biological life (or the organism and the social person), we consider emergent novelty, or the ideas of irreversible change as normal to everyday development (Barber 2008) (rather than a 'copying error' or anomaly) of physical, biological and social life (*ibid.*). This point frames the intelligibility of nature, integrating a coherent, logical necessary system of general ideas in terms of which every element of our experience can be interpreted (Whitehead 1978). This potent rhetoric asks us to consider a framework in which we should consider all aspects of both the physical and social to be connected and reflexive beyond notions of determinism.

Ingold shares much of this sentiment with Prigogine, but Ingold, however, has taken the ideas of indeterminacy in nature and molecular science and has utilised the roles played by essentialism in the social sciences, which have profound implications for the archaeological study of prehistoric material culture (Ingold 2000). For example highlighting the divisions between the real and historical, moderns and pre-moderns, etc in generating problems that are now being subject to much critique of the dichotomies by new interdisciplinary projects that address the following issues:

a) Approaches to cognition (the work of Donald) grounded in rigidly held assumptions of mind-body, individual-society and other dichotomies that are considered to hold states far from equilibrium of co-evolutionary forms of life (Ingold 2000, Kauffman 1995)
b) Versions of cultural theory that 'attribute human behaviours to designs that are passed from one generation to the next as the content of traditions' and in this point, appreciation that no one ever will be 'modern' or 'pre-modern' in the ways that standard accounts of science and modernity claim (Gamble 2007, Ingold 2000, Descola and Palssen 1996, Latour 1993).

The temporal nature of Ingold's research highlighted above also makes possible some of the hypotheses that he has previously developed about human beings as 'loci of creative growth relationships' (2000), which are powerful critiques of the nature/culture dichotomies. He has suggested that much of what post-processual archaeologists call cultural variation may consist of variations in skill – not just techniques of the body, but capabilities of action and perception of the whole organic being (Ingold 2000). He provides a potent alternative for our closely held oppositions between the two paradigms in considering aspects of enskillment. These being particular forms of experience and states that, "becoming skilled in the practice of a particular form of life is not a matter of furnishing a set of generalised capacities (as was considered for memory by Donald), given from the start as compartments of universal human nature, with specific cultural content" (Ingold 2000, 23). Rather than being transmitted from one generation to the next by tradition, they are 'regrown, incorporated into the modus operandi' of the developing human organism/person through training and experience in the performance of particular tasks. This latter point will be particularly important for our consideration of the Neolithic case study on reconstructed sound as it suggests the work relating to sound-sense relations are contingent upon the experiences that are first brought to bear on the environmental setting. Ingold also considers the temporality of

the work at hand in recognising that factors such as perception, skill and embodiment are intrinsically linked with notions of an all encompassing 'task' environment. Here both present and past peoples have found themselves in a constant state of reciprocated experience and reflexivity in situating the practitioner from the start in the context of active engagement with his or her surroundings (Ingold 2000, 5). Ingold's work itself can be drawn analogies with the ways in which the sense of hearing and pulses of rhythm are experienced by the body. Ingold, in earlier work likened the rhythms of human activity resonating with aspects of other rhythmic phenomena, the cycles of day and night. What is important for this is that the world and the experience of it, is not separated from individual organisms, but rather is generative within which organic forms are located and held in place (Ingold 1990, 215), which means that to dwell within the world, we do not act upon it, or do things to it; we move along with it. Rather like the ways in which a musician works with the space in creating something which sounds desirable to his or her ears.

Situating Creativity

This work, like some of the problematizing of creativity and experience that Ingold suggests, forms a critique of the presuppositions held by the new and post-processual paradigms in archaeology. It has shown that there are in fact no issues or problems that can be solved from a context independant perspective and that the organism is bound up in all aspects of environmental and social implications for their actions (Ingold 2000). Probably the most important factor this is the use of creativity in nature. The indeterminate ways in which the organism is generated through the course of its development is vital for our understanding that traditional modes of interpreting material culture and evidence in the archaeological record are flawed at best. Creativity is now evident as a powerful critique of determinacy in nature and as essential for the way humans engage with their environment not based upon laws which seek to prove natural states as repetitive, stable and seeking a mode of equilibrium (Barder 2007, Bergson 1911) but rather as in a constant state of flux, movement and full of indeterminate outcomes (*ibid.*). Improvisation and creativity are, as we have considered earlier in this volume, vital and normal for the development of both cultural and organic life as processes are compelled to improvise, not because they are operating on the inside of an established body of convention, but because no system of codes, rules and norms can anticipate every possible circumstance (Hallam and Ingold 2007). There are gaps that exist between non-specific guidelines and specific conditions in the world and these are never the same from one moment to the next, and as such, the space for improvisation between the two becomes and imperative that requires that people respond accordingly (*ibid.*). This is potential and dynamic for spontaneity and improvised cultural and organic creativity.

My purpose in this chapter is to illustrate with two case studies the relevance of the approaches of Ingold, Prigogine and Koerner's approach to skills, creativity, improvisation and perceptions for research on human and animal interactions in the Neolithic Southern Britain (non cal 4000–2200 bc). By exploring the available archaeological materials relating to themes of perception and contingency in the ways

in which people in the past constructed their identities through their material things, we may be able to strengthen arguments for including creativity and improvisation amongst the normal and essential ways in which life develops. I will focus on the complex and varied depositional practices reflected in the archaeological record from a number of sites relating to bone and artefact assemblages. Much of the relevance of these case studies relates to their documenting a wide variety of practices reflecting distinctly regional identities (Bruck 2005; Edmonds 1999) which are being explored by Neolithic studies alert to the importance of regional creative variations in both settlement and depositional practices. My research has been particularly concerned with the cultural and ecological contingency of sound and sound-sense, as it emerges and ensures beyond the visual realm at states far from equilibrium. The overestimation of the importance of vision for perception and social relations in society can be called into question, since eyes require light to enable visual engagement (Ingold 2000). Hearing and the sound-sense however, do not depend upon the qualities of vision. Sonic illumination can be activated in darkness, indeed, darkness is sometimes considered to heighten our perceptions to sounds and sound sources (Blesser and Salter 2007; Ingold 2000). It is this enduring and contingent nature of hearing and sound-sense which situates it amongst emergent modes of being and mutual engagement within the natural and cultural environments of both the archaeological and social sciences.

The Neolithic: A Model of Indeterminacy?

Until recently, notions and discussions of Neolithic habitation in southern Britain were centred around the need to locate and define the nature and extent of the domestication of animals and plants (Hodder 2003). It hinged upon the conclusion that a revolution in the ways in which people organised themselves was inevitable and all encompassing (Johnson 1999) and that a grand narrative as to the interactions of these peoples was in fact part of the revolutionary process towards the domestication and control of nature (Bradley 1984; Thomas 1999). This paradigm concluded that an inevitable homogeneity of culture was essential to the advancement of civilisation and that its trajectory could be followed and predicted just as was the case with foundational laws relating to rational modes of enquiry (Koerner 2007; Jones 2007). The truth, however, is rather more complex as we have seen with the difficulties in accepting a separation of culture and nature. We are aware that nothing exists as context independent, and that novel and unpredictable alliances are the natural and essential ways of the world. The struggle between the laws of nature and the dialogues of culture are plain to see in light of the presuppositions imposed upon the discussions of the separating theoretical frameworks of the 'new' and post processual archaeology. In light of this, I propose that the evidence pertaining to the Neolithic past must be subject to the same scrutiny about the supposed predictability of cultural trajectories surrounding material practices. As the following case studies will show, they yield up archaeological evidence which does not reflect uniformity in the way of things relating to Neolithic depositional practices, but rather they are contingent upon the creativity and rich variability to which these communities must have held dear. They reflect what Ingold suggests as perception and dwelling in their cultural practices which bring to bear their relationships with the animal world.

Etton Causewayed Enclosure, Cambridgeshire

The earlier Neolithic enclosure at Etton, Cambridgeshire (fourth millennium BC onwards) is an intriguing example of how perception of ecological engagement is reflected in the varied and rich depositional practices of Neolithic peoples. During the 1980's the site was excavated ahead of quarrying extension near Peterborough and revealed a large enclosure of multi phased use (Edmonds 1999, 110; Pryor 1999). Their ditches were recut on a number of occasions and the habitation of the site was thought to be contingent upon the cyclical nature of the seasons (*ibid.*). It is a site which must have held a rich tradition of creative interaction with the environment as was located on the Cambridgeshire fens, a waterlogged ecology for part of the year (Pryor 1999). Depositional and habitation practices at the site led the archaeologist, Francis Pryor to conclude that there must have been two symbolic zones, one associated with the living and the other with the dead (Edmonds 1999, 110). The ditches to the enclosure had been recut on a number of occasions and were home to a rich array of deposited items both discarded and deliberately and carefully placed (*ibid.*). This latter point is important as it suggests the habitants of the enclosure are not just discarding their material items, but rather are engaging in practices which are both symbolic and culturally profound. The importance of this to our discussion is self evident, it suggests that peoples at this site were fully aware of their place in the cosmology of their locale and they were engaging in creative ways with their surroundings by placing a variety of artefacts, animal and even human bones in the waterlogged ditches surrounding the enclosure (Pryor 1999). The nature of the depositional practices were varied, with what are considered to be rather more exotic items being placed with what we would consider more everyday items. Smashed stone axes of Cumbrian origin, flint tools and pottery intermingled with bundles of fresh cattle bones places on birch bark mats and disarticulated human bones mixed with broken cattle bones were yielded from multiple layers of occupation (Pryor 1999). It can be difficult to ascertain the motives for depositing items in this way, but what is self evident for me here, is that such a package of cultural practices did not flourish devoid of their environmental contexts. The people would have been more than aware of the changing nature of their environment and they would have almost certainly have created and recreated ways in which the past could be remembered through the renewal of the land, seasons, and their interactions with their animals. The evidence suggests that human animal interactions were of the utmost importance to these early communities with the artefactual remains pointing towards a culture based upon the contingency of blurred and intertwined identities (Lewis-Williams and Pearce 2005). It seems that distinctions were not made between the natural resources utilised as part of domestic and symbolic events and the cultural identity placed upon the material from the point of view of cosmology. This results in an understanding of the evidence as emergent and creative, far removed from our long held beliefs in the nature/culture agrios/domus paradigms.

The role of the animal human interaction is potent for the case of Etton. It allows us to gain somewhat of an understanding of the ways in which the communities may have used animal remains. Their perceptions of their environment led them to weave creative, transformative concepts associated with material culture that goes beyond

our previously held assumptions about the arbitrary nature of the Neolithic in Britain. What are laid bare with this case study are our previously held assumptions about the normative and the particular in interpreting the evidence based on the processual or post processual paradigm. The evidence does not conform to either of the dichotomies as it is:

a) contingent upon the unification of both the Neolithic inhabitants engagement with their animals on a daily and seasonal basis
b) dependant upon the creative nature by which human/animal interactions and identities are subject to innovative and unpredictable change.

Etton is a potent example of mapping social geographies relating to ecological enlightenment in Neolithic communities. It suggests that previously held assumptions about the presuppositions of a Neolithic 'package' are false and that cultural diversity is not made up of equally small parts which are subject to a single trajectory towards a balanced equilibrium as is suggested by foundational natural laws. They are rather subject to the creativity that is essential for the development of both organic and cultural life (Koerner 2008). We come back to Ingold's work on the creativity inherent in all aspects of life to explain some of the movements and developments within the scrutiny of human/animal interactions of the Neolithic. Ingold places a precedence on the spontaneous nature of creative interactions by suggesting that within anthropological (and indeed archaeological) study, creativity can best contribute by challenging, rather than reproducing – the polarity between novelty and convention (Hallam and Ingold 2007). This is pertinent for our discussion on creativity in Neolithic depositional practices as it also challenges our assumptions regarding the separation of novelty and convention in archaeology. It gives us new ground from which to engage with the evidence that places our interpretations firmly in the framework of response to lifes contingencies (*ibid.*). If such contingencies were therefore the case, can we not also consider the concepts of adaptive reuse (Bradley 1993). Some of the material remains recovered from such Neolithic contexts as enclosure ditches, post holes and other sites such as the Coneybury 'anomaly' or later Neolithic sites such as Durrington Walls (Thomas *forthcoming*, Thomas 1999, Bradley 1998) where seemingly unusual assemblages of both domesticated and wild animals were brought together with fragments of pots and stone tools, could be reinterpreted utilising creative explanations as to the use and reuse of some of the organic remains.

New Alliances of Experimental Archaeology – Research Case Study: Acoustic archaeology and the contingency of transformative sound

In considering some of the alternative and adaptive reuses of material and organic remains – I would like to turn the attention of the discussion to some of my own research into possible uses of animal remains relating to sound and sound transformations. My work has been involved with how our Neolithic ancestors may have interpreted their worlds through the use of sensory contingent things. In this instance sound, as we have discussed, is essential to our own modes of communication (Marshall 2009).

Figure 4.1. Deposits from Durrington Walls, Wiltshire (Permission J. Thomas).

Experimental forms of archaeology today are subject to the theoretical modes of enquiry that have been discussed when considering some of the historiography of the separation of culture and nature through the paradigms of the new and post-processual archaeology (Koerner 2008). I will outline here, some of the motivations in setting the case for the integrated view of sound and acoustics in their Neolithic setting along with the theoretical frameworks relating to our understanding of the experiential aspects of acoustic engagement with sound environments and their effects on the body and mind. This latter part is important if we are to understand how past communities may have found the qualitative differences of sound as transformatory.

Acoustics and Space

In realising an experimental form of archaeology in terms of our freedom of expression in undertaking the task, we must first understand some of the concepts which allow for our perception to be expressed both creatively and innovatory with spaces and surrounding acoustic events. The relationship between the auditory system and the cultural spaces we inhabit is vital to mention if we are to recognise a sensory experience that is classed as 'whole' (D'errico and Lawson 2006). By this I refer to the effects of sound in space when the organism is present (the human actor) with these spaces

identified through sound and so called 'Sound Horizons' (Blesser and Salter 2007), that is, sound parameters being the catalysts by which social space is negotiated and demarcated (Watson and Keating 1999; Watson 2001). These horizons are themselves novel as they are interactive and indeterminate as the actor engages with the space. They are, in effect both inclusive and exclusive as acoustical responses interact in creating an auditory dialogue (Blesser and Salter 2007; Cross *et al.* 2002), becoming part of both the instrument and the participant during the course of engagement.

The human experience of sound is, as with other aspects of the sensory realm, context dependant (Blesser and Salter 2007) with the interplay of both ecological creativity and cultural perception working together in choosing the individual reactions to sound horizons (*ibid.*). These implications are somewhat integral to the understanding of the use of spaces aurally in the prehistoric past and for the purpose of this discussion, the Neolithic past. I will mention a broad outline of some of the acoustical phenomena associated with the experience of sound that are likely to be present when conducting some of the experiments. To understand how our Neolithic ancestors may have engaged with monuments such as passage graves, chambered tombs and stone circles, we must first consider the types of acoustical phenomena that such structures could have produced when sound was generated within them. Pilot studies at Neolithic monuments in the British Isles (3800–2000 bc) have suggested that these places were conducive to the creation of dynamic multi-sensory experiences, affording acoustic effects such as echoes, resonance, and Standing Waves (Watson and Keating 1999, 326). These effects may not have been intentional in the design of the structure, it is highly unlikely that they would have been ignored by the communities that used them in prehistory (Watson 2001; Tuzin 1984). Understanding of acoustical effects within an enclosed space today is far removed from the interpretation that peoples in the Neolithic may have attributed to monuments (Waller 2004; Tuzin 1984). But what such an engagement represents for ideas of emergent novelty and complexity, given the reciprocal relationship that exists between the organism and the given effect is a potent reminder of the creative and indeterminate nature of interaction. Studies of the tangible effects of auditory phenomena over the last 100 years has seen the development of a range of methods for the quantification of the acoustics of different physical environments (Waller 2004; Watson and Keating 1999) and as such is entrenched in the Western scientific tradition where the consensus has been to reduce the effects of undesirable acoustical features such as standing waves, flutter echoes and resonances (Parncutt 2001; Needham 1967). When current approaches to acoustics are quantified using calibrated measurements in the modern sense, the quantification tends to be carried out with a view to minimising resonances and flutter echoes rather than exploring ways in which they could have been used positively in prehistory (Cross and Watson 2005; Watson and Keating 1999). With this in mind, I would suggest that the experiential aspect of a space that exhibits auditory phenomena and the analysis of the resulting phenomena psychologically could be employed in conjunction with quantifiable methods of sound measurement to interpret acoustical qualities that may have been noticed and utilised in the Neolithic.

The Experiments

For the purpose of our sound experiments, a number of instruments from organic materials originating from cows were constructed. They consisted of a number of blast horns from various breeds of traceable ancient cattle, drums made by stretch calfskin over a wooden base, flutes made from the long bones of cattle and whistles from cattle toe bones. These were then analysed using spectrum analysis software to ascertain fundamental and frequency tonal harmonics so that we could benchmark the qualitative differences before recording these within monumental settings. The instruments themselves were also analysed to consider the fundamental frequency tones and residual harmonics in both open and closed spaces. There were a number of intriguing results which were not expected which shows consistency with our approach to the indeterminate nature of acoustical responses in specific spaces.

The fundamental frequencies were located and could be predicted for each of the instruments, but when played in some of the spaces chosen for our experiment – in this case, Bryn Cellu Ddu on Anglesey and a number of Neolithic and early Bronze Age sites of the Kilmartin Valley complex, Kilmartin, South Western Argyle – the sound responses yielded from the experiments displayed unusual residual effects. These consisted of standing waves in the case of Bryn Cellu Ddu which resulted in a distorted assumption of the location of the sound source. The Kilmartin context also exhibited a number of strange phenomena outcomes, for example the player would sound inside the stone circle of Temple Wood monument and the resulting acoustical effect gave the impression of more than one instrument responding, the sound waves bouncing off certain stones and promoting a qualitatively different effect. Another site of the Kilmartin complex, the Nether Largie Cairn South also exhibited more acoustic phenomena than was expected in such a small space. Prominent Infrasonic phenomena (which is the production of sub harmonic frequencies below the threshold of human hearing) and flutter echoes (flutter echoes are caused by echoes bouncing back and forth from parallel surfaces following a percussive sound such as a clap or beat of a drum) were yielded from the small chambered space inside the cairn that could again be considered to sound like more than one instrument was being played. This happened with both the percussion and low frequency timbred horns. Such effects were not accounted for in our predictions of how the sound would perform in spaces, we could not account accurately for these based on the foundational laws of architectural acoustics (Sabine 1964). This led us to conclude that we also altered the ways in which the instruments were played to compensate for the indeterminate effects, our own auditory systems were engaging in a novel interaction with the space to produce effects that could neither be accounted for, nor predicted given the size and orientation of the space. We could only conclude through the use of the sounding devices in the space that we were experiencing powerful example of complex emergent improvisation and creativity in sound reciprocity.

Figure 4.2. Reconstructed instruments for on site tests (Copyright, the author).

Figure 4.3. Reconstructed instruments for on site tests (Copyright, the author).

Psychoacoustic Phenomena and Transformation

These tests were vital in bringing to bear the effects on aspects of the transformative qualities of sound in given environmental settings.

I would like to mention also how some of these specific auditory phenomena affect the human physical body which can result in experiences that give the impression of the unfamiliar, powerful and transformatory In reality this is the body language of sound, its personality if you like in the ability of sound to interact according to spatial dimension. While the detailed and subtle effects of sound in general on consciousness are still subject to investigation, some broader effects, both psychological and physical have been recorded (Blesser and Salter 2007). Most of the human body resonates at low frequencies and what is intriguing from the point of view of spatial movement, is that the body resonates at various frequencies depending on what position it is in (Deveraux 2001) for example whether an individual is lying down, sitting or kneeling. This could therefore have implications for those peoples who may have used enclosed chambered spaces to conduct their ritualised transformative activities (*ibid.*). Physical sound itself is a pressure wave that transports both sonic events and the attributes of a acoustic space to the listener (Watson 2007), thereby connecting the external world to the listeners ears (Blesser and Salter 2007, 12) as this notion suggests, it is this activation of space which is essential in understanding the transformation of sound through the human/space interaction and creative interpretation. Here the biology and cultural filtering works together to create responses to aural space that cannot be accounted for if we were to invoke laws relating to either normative or particular science.

Probably one of the most powerful results of the use of sounding devices in enclosed, low ceiling spaces is infrasound. It has recorded some of the most psychologically distinct effects on human participants in a complete chambered space (Deveraux 2001, Watson and Keating 1999). It occurs at the low end of the frequency spectrum, below the threshold of human auditory system (which terminates at approx 15–20 Hz depending on the individual). Sub frequencies and infrasound can occur simultaneously, but infrasound can also be present on its own. It is usually felt as a low chugging effect and can affect the inner ear by creating lower harmonic distortions (Watson 2007; Cross and Watson 2001). Certain specific effects of moderate intensity infrasound have been recorded, one is the sensation of dizziness and motion (Reznikoff 2007). The result is a feeling of nausea, a vertigo like sensation that could well have been interpreted in the past as engagement with other worlds, the space literally as a gateway to transformation from the physical world to that of the ancestral domain with the sounding devices as the key (Blake and Cross 2008). At between 2–5Hz, the physical effects of infrasound on the human subject include a pressure build up in the middle ear difficulty in swallowing and speaking, chest wall vibration and post-exposure headaches, gagging sensations and watering of the eyes (Cross *et al.* 2002; Deveraux 2001). Whilst we are aware of these as physical effects that have taken place in a modern control environment, it is entirely feasible that a similar set of auditory phenomena could have been attributed to the symbolic or magical ways in which these symptoms could have been generated using such sounding devices (Deveraux and Jahn 1996). Is it then, any wonder that the Neolithic peoples may have coveted their cattle in special ways as their purpose was not

just a physical show of conspicuous wealth. But of a framework of interrelated social bonds through the reuse of the remains in the construction of devices of transformation? It is pertinent to suggest that the people involved in such rites of identity would have had their own explanations for why such effects occurred during their most intimate of events. These effects on the body are indicative of the transformatory nature of sound and sound-senses from the point of view that creativity is interacting with the space itself, the space becomes part of the instrument and is therefore alive with the experience of sound. In effect, the environment becomes part of the organism of sound and is given life as an active part of the creative process.

This latter point takes us back to Ingold's directions in calling for the barriers to be broken down to separate notions of what it is to inhabit and 'dwell' within a space or environment. These examples of the use of sound environments call into question the long held presuppositions over the belief in a Neolithic that was populated by peoples wishing to remove themselves from their natural world and engage in a systematic control of the ecology around them. Rather it asks us to call into question our own interpretation of that world which increasingly seems as if their contemporary communities were in fact working with aspects of nature that are quite clearly reflected in the visible archaeological record. The use of animal remains and its variation by region allow us to reinterpret some of the Neolithic evidence from the point of view of emergent creativity. These actions seem to be happening in working with a cosmology that does not upset the processes of interaction between the natural world and the cultural inhabitations of that world on the part of the community, but rather their actions are contingent upon ways in which that world is manifest through ecology and vice versa. Their interactions with that world were subject to subsequent factors as: ecological engagement, mnemonic triggers of past experiences, skills of everyday manifestations of material culture and sensory perception of sound, taste, vision and touch.

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5. Experimental Archaeology After Simplicity – Implications for Reflexivity of Insights that a 'Common World' is not 'Given'

Stephanie Koerner

Abstract

During the latter decades of the 20th century it is unlikely that many archaeologists would have embraced suggestions that experimental archaeology might be highly relevant for fresh orientations towards apparently irresolvable clashes between the most influentially opposed so-called 'new' and 'post-processual' theoretical paradigms. This chapter explores something of the range of developments that are bringing about remarkable change in this situation.

1. Introduction

> A common world is not something we can come to recognize, as though it had always been here … A common world, if there is going to be one, is something we have to build, tooth and nail together (Latour 2004, 455).

Until quite recently, it is unlikely that many researchers would have been receptive to the suggestion that experimental archaeology could provide contexts for exploring the relevance of the idea in the passage above (Latour 2004) for 'going beyond' paradigms for human agency and history, which have been grounded in such dichotomies as those of nature-culture, science-values, moderns – pre-moderns, reason versus tradition, and, especially, the "global and universal versus the local and illusory" (cf. Rorty 1979; Godelier 1986; Ingold 1993, 2000; Fardon 1995; Descola and Pálssen 1996; Cowan *et al*. 2001). Such negative responses should not be surprising. These include connotations having to do with the importance of creative trying new things out and re-designing prevailing relationships, processes and entities for understanding and sustaining plurality of human-environmental relationships (cf. Callon 1986; Ingold 2000; Latour 2008; Koerner and Szersynski 2009).

There are numerous further reasons for negative responses to the suggestion mentioned at the onset. The implications of appreciating that a 'common world' is not 'given' go against the grain of the key presuppositions about experimental practices

and science, which have been shared by archaeology's most influentially opposed 'new' (and/or 'processual') and 'post-processual' paradigms. These implications include that:

a) There are no such things as context independent problems. "We never experience or form judgments about objects and events in isolation, but only in connection with a contextualised whole. The latter is called a situation" (Dewey 1938: 66–67),
b) Complexity and emergent novelty are the normal state of affairs for reality, and crucial for understanding how we find the world intelligible.
c) Dichotomies, such as those of science-values, nature-culture, real-historical, modern – pre-modern, are both historically contingent and as real as bricks, prioritise the least rather than the most tractable problems, and impede appreciating the importance for sustaining diversity of human life ways of plurality of the past and future aspirations.

With regards to problematical shared presuppositions, particularly tenacious have been:

a) that the primary task of science is to reducing the plurality of context dependent experiences and phenomena to universally valid concepts, categories and propositions ('foundational laws), whose explanatory force is independent of any context of application,
b) about the ultimate simplicity of the 'common world',
c) about obstacles, which are said to be posed for attaining universalisable knowledge of (and/or for predicting, controlling, and instrumental employment of) immutables, by such things as problems with technological instruments, social interests and cultural traditions.

Today (as several contributions to the present volume show) the situation is undergoing remarkable change. My main purpose in this chapter is to highlight something of the range of the developments involved. Emphasis falls upon developments relating to:

a) the relevance of revisiting the polemical disputes over experimental archaeology of the 1970s and 1980s for fresh approaches to the tenacity of nature-culture, reason-tradition and reality – historically contingent dichotomies (e.g. Ingold 1993, 2000; Descola and Pálssen 1996),
b) that there have always been sophisticated alternatives to vexed options of determinism and relativism (Toulmin 1990),
c) considerable changes that are taking place in the ideas and goals of experimental archaeology – with several bearing directly upon current interdisciplinary interest in new approaches to questions about how organic and cultural life forms emerge, are transformed and endure in states far from equilibrium (e.g. Whitehead 1978; Prigogine and Stengers 1984; Allen 1989; Van der Leeuw 1989; Kauffmann 1995; Prigogine 1997; Stengers 1997; Hallem and Ingold 2007).

Part 2 explores backgrounds of dualist paradigms for human agency, history and knowledge, and influentially opposed perspectives on experimental archaeology. This will help us to contextualise challenges these perspectives' shared presuppositions pose.

Writing on the tenacity of dualist categories, Toulmin (1990: 147) notes that: "since the 1980s, it has become a common place" to say that we need to reintegrate humanity [and "human agency"] with nature [and "causal interactions" of physical and life processes] and find places for these within an ecological account of the larger world. But to this day, many people are unwilling to give up this separation of Human Nature from Material Nature." Similarly, writing on the tenacity of essentialist objectivism and subjectivist relativism, Trigger (2004: 46) notes that when polemical theoretical debates continue with "no signs of closure, it is worth enquiring whether the wrong questions are being asked." Part 3 concerns the relevance for fresh critical and constructive experimental archaeologies of exploring some of the paradoxical presuppositions such debates perpetuate.

We will need to boldly underscore **contextual circumstances** throughout our considerations for a number of reasons. Many aspects of today's efforts to avoid these debates are not at all new. Writing on "ways ahead" against the grain of the most polemically opposed paradigms of the "modern cosmopolis," Stephen Toulmin (1990) stresses that there have always been valuable alternatives. Several current experimental archaeologies share features with long traditions of alternatives to (a) essentialist conceptions of supposedly context independent determinants of nature and society, and (b) subjectivist interpretations of 'cultural constructions' as historically contingent and, even, as somehow illusory and/or not real. In alternatives to these vexed options – as in a number of approaches represented in this volume – appreciating that a 'common world' is not 'given' does not make it unreal. Historically contingent processes, relationships and agencies that endure in states far from equilibrium are as real as bricks. Unfortunately, such insights have often been eclipsed under circumstances where **some** are allowed to supposedly settle 'crises over representation' in ways that

a) reduce issues of trust to the supposed 'laboratory conditions' of expert competence
b) hinge upon what Toulmin's (1990) calls the 'myth of the clean slate'
c) disregard the intrinsic plurality of sustainable human-environmental relationships.

Another reason to boldly underscore historical contingency, is to stress that such circumstances are neither unique nor part of anything like a uni-linear continuum. In Part 4 we consider developments that are encouraging critical awareness of these situations, as well as fresh orientations towards experimental archaeology. Emphasis falls upon developments that relate most directly to concerns to 'go beyond' conceptions of human agency and history, which perpetuate dualist characterisations of nature versus culture, moderns versus pre-moderns and "the real versus the historical" (Ingold 2008). These developments are not only of academic importance. The chapter concludes with examples to show how they relate to efforts to integrate strongly reflexive experimental archaeologies into projects to sustain and enhance plurality of "lived cultural heritage" (Erickson and Chandler 1989; Cleere 2006; Wescoat 2008).

2. Revisiting the Roles of Experimental Archaeology in the Parting of the Ways of Influentially Opposed Theoretical Options

With the exception of "landscape" (Layton and Ucko1999; Ashmore 2008; 2004; David and Thomas 2008), few areas of specialisation have been cited more often than experimental archaeology in debates over influentially opposed theoretical paradigms and their positions on whether archaeology can or should aspire to be a science (for instance, Pinsky and Wylie 1989; Trigger 1989; Yoffee and Sherratt 1993; Mathews 1999; Meskell and Preucel 2004). Robert Preucel's (1991) description of contrasts between the "ways of knowing the past" of "three influential theoretical programmes in Anglo-American archaeology" brings this theme into relief.

a) "Positivist programs" ("New" and "processual"), he says, seek to "explain empirically based observational statements in terms of general laws" (Preucel 1991: 18). Lewis Binford argued for the relevance of the logical positivist idea that "a causal explanation of an event involved deducing a descriptive statement of the event from one or more general laws in connection with certain unique statements about initial conditions" (Preucel 1991, 19). For Binford and others (e.g. Watson et al. 1971) "the covering law model of explanation and testing methodology [were] hallmarks of scientific archaeology" (Preucel 1991, 19; see also, Binford 1972, 1977, 1978, 1982).
b) "Critical theory" grew out of sociological traditions that have been concerned especially to reveal how ideology legitimates social inequalities and injustices (Althusser and Balibar 1970; Habermas 1972; Leone *et al*. [1987] 1999; Preucel 1991, 24). Mark Leone has been one of its most influential exponents. Trained initially as a processual archaeologist, Leone became concerned with impacts of ideologies on archaeological reconstructions of past societies, "Leone's work of the late 1970s sought to expose the ideology inherent in the modern tripartite subdivision of time into past, present and future" (Preucel 1990, 24), and associated dualist characterisations of science and values.
c) Hermeneutic programs share an "interest in eliciting meaning through interpretation" (Preucel 1991, 21). "In the late 1970s Hodder, a former student of David Clarke [1968, 1973], experienced an intellectual crisis that caused him to re-examine the philosophical basis of processual archaeology" (Preucel 1991, 21). The dilemma for archaeologists, Hodder said, is that there is a "widespread desire for science and objective texts, a fear of speculation and the subjective.... Yet to say anything about the past, and past ideas, involves moving beyond the data to interpret them, and there can be no testing of these interpretations because the data are part of the same argument as Yet to say anything about the past, and past ideas, involves moving beyond the data to interpret them, and there can be no testing of these interpretations because the data are part of the same argument as the theories" (Preucel 1991, 21–22; see also Hodder 1982).

Preucel went on to say that at issue are such questions as:

> Do archaeologists discover an objective past? Or do they create alternative pasts? Is archaeology properly considered a human science or a natural science? In the course of

> dealing with these and related issues, archaeology has turned once again to philosophy for guidance. Just as positivism was adopted by processual archaeology in the 1960s, post-positivism is currently being embraced by the movement now known as post-processual archaeology. The post-processual movement is signified by an attack on the scientism of processual archaeology (Hodder 1982; Shanks and Tilley 1987) and the exploration of alternative interpretive frameworks (Preucel 1991, 17; see also Leone *et al*. [1987] 1999).

Bruce Trigger's *A History of Archaeological Thought* (1989) suggests that one of the reasons why experimental archaeology became the focus of so much disagreement was its associated with "scientistic" approaches to that:

> Archaeology is the only social science that has no direct access to information about human behaviour. Unlike economists, political scientists, sociologists and ethnologists, archaeologists cannot talk to the people they study or observe their activities. Unlike historians they have no written accounts of what human beings thought or did in prehistoric times. That must be inferred as far as possible from the remains of what they made and used (Trigger 1989, 357).

Writing on "Testing, Middle Range Theory, and Ethnoarchaeology," Mathew Johnson (1999, 62–63) said that the history of experimental archaeology shows the divergence in ethno-archaeology between "in many ways mutually contradictory paths": (a) one pursues "links between behavior and the archaeological record that will help us to look at such processes in all times and places," (b) the other stresses the "need to understand beliefs." Contrasting pursuits with regards to archaeological knowledge imply contrasting perspectives on human agency and history. For Johnson:

> Which approach offers the more convincing way forward depends very much on the general theory of society that one finds convincing. Can one generalize between societies? Are societies to be seen as systems fundamentally adapted to their environment, or is symbolic meaning important? (Johnson 1999, 63).

These patterns relate to further divergences, including those arising out of opposed interpretations of science and modernity as either a triumph or as a tragedy, which have roots in Enlightenment and counter-Enlightenment pedagogical and political ideals (cf. Collingwood 1949; Lowenthal 1985; Daston 2005). Michael Friedman's (2000) illuminating book on the "divergence" of "analytic and post-analytic" and "continental" philosophical traditions indicates that there are deep connections between opposed visions of modernity, and processual and post-processual archaeological programmes (cf. Trigger 1989; Koerner 2008). Already in the 1970s, critics noted the tenacity of such divisions. For Pierre Bourdieu (1970), of all the "artificially" dividing oppositions "the most fundamental and the most ruinous" is that between "objectivism and subjectivism." In Bourdieu's view, "the very fact that this division constantly reappears in virtually the same form" suggests something of the presuppositions opposed views share, and of how they eclipse valuable alternatives, including by treating these as though they were mere new versions of relativism. In the 1980s John Barrett ([1988] 1999, 1994, 2000) examined impacts of such divisions on predominant conceptions of the archaeological record and human agency. He stressed the bearing upon problem of Linda Patrik's exploration of the question, "Is There an Archaeological Record?" ([1985] 1999). For Patrik (1999, 119), archaeology's influentially opposed paradigms could be

characterised in terms of two models ("physical and textual") of what they "do to or with the 'archaeological record'". One model envisages the record as fossilized (static) evidence of past (dynamic) natural and social processes. Exponents of this "physical model" archaeologists "apply scales to, give meaning to, extract information from, carry out experiments to test hypotheses against the record" (Patrik 1999; Binford 1982; Watson *et al*. 1971; Schiffer 1976). One problem with this model is its assumption about direct mechanical relationships between "imprints in the archaeological materials [and] dynamic past processes" (Barrett 1988: 5). In the "textual model… the record consists of physical objects and features that are material signs or symbols of past concepts" (Patrik 1999, 127), and what archaeologists "do to or with the record" is said to depend on cultural conventions for interpreting the past (Hodder 1982) and on aims to "discover" meanings, "translate" ideas and "read" symbols (Patrik 1999, 140–141). For Barrett ([1988] 1999, 1994) and quite a number of other archaeologists and anthropologists (Rosaldo 1989; Dobres and Robb (ed.) 2000; Gardner (ed.) 2004), both options imply problematic conceptions of human agency. Amongst other difficulties, agency is envisaged paradoxically both as a crucial cause of social change and as mere node through which 'systems' supposedly pursue states of equilibrium (Koerner 2004). Writing on the relevance of appreciating the indeterminacy of physical, organic and social processes for fresh approaches to such difficulties, Gregiore Nicolas and Ilya Prigogene note that:

> Our everyday experience teaches that adaptability and plasticity of behavior, two basic features of non-linear dynamic systems capable of performing transitions in far from equilibrium conditions, rank among the most conspicuous characteristics of human societies…. A basic question that can be raised is whether, under those conditions, the overall evolution is capable of leading to some kind of global optimum, or, on the contrary, whether human [behaviors, communities and histories involve] complex stochastic processes whose rules can in no way be designed in advance. In other words, is past experience sufficient for predicting the future, or is a high degree of unpredictability of the future the essence of human adventure, be it at the level of individual learning or at the collective levels of history making? (Nicolis and Prigogine 1989, 238).

The issues at stake have important ethical implications. One of the themes that runs through Prigogine's book, *The End of Certainty, Time, Chaos and the New Laws of Nature* (1997, 55) is that "as long as science led to the description of nature as an automaton, ideals of truth and human freedom were contradictory." Similarly, Hannah Arendt (1977 [1961], 147) argued that the removal of moral freedom from the material physical and social order ('out there') to the inward domain of individual mental states may have played crucial roles in "the privatisation of freedom and ethics". Or put another way, it may only have been after it had become possible to describe "nature as an automaton" (Prigogine 1997), that influential images of humanity came to:

a) envisage the rational individual 'subject' as the source of all meaning and value,
b) reduce social life to inter-individual contractual structures (Koerner 2004),
c) dichotomise matters of fact versus matters of concern (Latour and Weibel eds 2005).

Unfortunately, all too often such themes have been interpreted in terms of vexed options of objectivism-subjectivism, moderns – pre-moderns, the real versus the historically contingent, and so on. This section outlines something of the background of presuppositions about science and the world, which perpetuate such dichotomies. In Part 3 we consider several especially problematic presuppositions in more detail.

2.1. "Timbers of the Modern Cosmopolis" (Toulmin 1990)

For quite some time, the humanities and social sciences have been described as being in a state of ferment on numerous theoretical, political and ethical issues. Whilst few would describe their fields as static prior to the 1960s, many stress rates of change being greater than ever before. Some of the most controversial debates over "new paradigms" have centred on concepts and categories that were at one time treated as 'given' (e.g., Kuhn 1962; Ardener 1973; Clarke 1968, 1973; Salmon 1982; Binford and Sabloff 1983; Pinski and Wylie 1989; Preucel 1991; Yoffee and Sherratt 1994; Rowlands 2004; Thomas 2004). Especially categories that form dichotomies of nature-culture, mental-material, facts-values, and so on, are said to have been previously taken for granted. Since the 1960s, precisely such categories have come under convergent forms of scrutiny in an extraordinary diversity of physical, life and social sciences, and humanities (Augé 1977; Wolf 1975; Fabian 1983; Shapin and Shaffer 1985; Latour 1993; Daston 2000; Latour and Weibel 2002, 2005). Today there is a vast literature motivated by awareness that, especially, the categories on which so-called 'standard accounts' (or 'meta-narratives') about science and modernity have hinged, are products of historically contingent circumstances.

For over three centuries, dichotomies of nature-culture have functioned as what Stephen Toulmin (1990) calls the "principle timbers" of the "modern cosmopolis" (cosmological and political framework) and associated debates over such pedagogical and political issues as:

a) whether the humanities and social science can or should aspire to meeting the requirements of a reductionist science (that is a science that seeks to reduce the diversity of observations to what many physicists call 'foundational laws' of immutable entities and processes) (Trigger 2004),
b) the relative merits of the most influentially opposed approaches to objects of study, methodologies and modes of explanation or interpretation, relationships of researches to their objects of study, and relationships between such contents of research and wider historical contexts (Preucel 1990; Stone and Molyneaux 1994; Meskell and Preucel 2004),
c) interpreting science and modernity as a triumph or as a tragedy, and conditions of possibility for "cross-cultural translation" – in particular, "translation" (and "representation" of so-called traditional and modern "world views" (cf. Asad 1986; Ingold 1993; Daston ed. 2000).

A striking recurrent feature of 17th and 18th century pedagogical and political culture was what Toulmin calls the "myth of the clean slate" – the idea that "overcoming" the destructive impacts of war required building a new social order and material world "from scratch." The "principle timbers" of this idea were separations of "sciences of Nature from fields concerning Humanity" (Toulmin 1990, 99–101) The iconic figures

of Enlightenment experimental science and mechanical natural philosophy were divided between those who argued against and those who argued for the possibility of a 'science of man.' In the latter views, such a science was possible on grounds of immutable determinants of human behavior and society (Nature and/or Reason), and on such premises as the following.

a) Humans beings, as unique as they may seem, are none the less as natural as any other bodily-intelligent being in their obedience to Newtonian laws. They are like any body which 'endeavors to persevere in the present state', that of living.
b) The forces of self-preservation that function as the 'principles' of Newton's 'laws of nature' should also be the focus of science of man.
c) The method to discover these principles must be a Newtonian one, i.e., that of social physics.
d) From observations on 'secondary' qualities of human behavior and cultures, the social scientist must infer the 'primary qualities' of human nature (since these do not vary across time and space).
e) The language used to explain human nature must be likewise 'natural' rather than 'human'. (That is, it be a language of immutable quantifiable properties – based on terms like universal 'springs of human action' and 'social order' – rather than varying cultural contexts) (Mali 1992, 26).

Efforts to satisfy these requirements often led to curious images of individual psychology and society based on themes in Newton's *Principia Mathematica* (1687) and John Locke's (1632–1704) *Essay on Human Understanding* ([1689] 1975), consisting of: "an atomic psychology, which explained (or explained away) mind as a mosaic of sensations and ideas linked together by 'laws' of association" and an "atomic sociology, which reduced society to a cluster of human atoms, complete and self-contained each in itself and only mutually attracting and repelling each other" (Koyré 1965, 22). Counter Enlightenment romanticism objected to the very idea of a 'science of man.' But as Toulmin points out,

> Romanticism never broke with rationalism: it was rationalism's mirror image. Descartes exalted a capacity for formal rationality and logical calculation as the supremely 'mental' thing in human nature, at the expense of emotional experience, which is a regrettable by product of our bodily natures. From Wordsworth or Goethe on… nobility attached a readiness to surrender to the experience of deep emotions… [and the supposed incommensurability of cultural traditions]. This is not a position that transcends dualism… but votes for the opposite side of the dichotomy (Toulmin 1990, 148).

During the 19th and 20th century, the "nature-culture dichotomy became a central dogma, providing a series of analytic tools for apparently antithetical research programmes" (Descola and Pálssen 1996). In the 18th century divisions between human beings' natures and cultures were treated as obstacles to "what would be called social science" (Cassirer 1960). By contrast, in the late 18th century, the nature-culture dichotomy became interpreted as a universalisable causal force in human history. During the 19th century, dialectical images devised by Kant (1963 [1784, 1997 [1778]], Hegel (1975 [1831]), Marx and Engels (1975 [1846]), and others were given central roles in the schemes, which the founding figures of anthropology, archaeology and

sociology devised in order to: (a) distinguish their aims from 'antiquarianism,' (b) satisfy requirements of a science, (c) eventually be able to classify all ethnographically, archaeologically and historically documented societies, and explain their differences in cultural evolutionary terms (Morgan 1877 [1966]; Tylor 1871 [1958]. In the 20th century, the dichotomy played essential roles in the ways in which "modern" paradigms distinguished themselves from their so-called "classical period" predecessors and from one another. According to influentially opposed modern paradigms, problems with predecessors and contemporary opponents were due to conflating the "natural order" and "social order" (Durkheim 1895; Boas 1940; Radcliffe-Brown 1948; Hollingshead 1940). For instance, in an essay that greatly influenced Julian Steward (1955) and neo-evolutionary approaches to ethnographic and archaeological analogy (e.g., Sahlins 1958, 1976; Service 1962, 1976), August Hollingshead argued that to "get on the right track" distinctions between the ecological and the social orders needed to be sharpened (see Pálssen 1996).

> The former is primarily an extension of the order found everywhere in nature, whereas the latter is exclusively, or at last almost, a distinctly human phenomenon.... The ecological order is primarily rooted in competition ['natural selection'], whereas social organization has evolved out of communication (Hollingshead 1940, 358).

Then – as (in many contexts) now – nature-culture, explanation-interpretation, modernity-tradition, reality-historical contingency and related dichotomies divide approaches to such key methodological issues as:

a) identifying and describing objects of study,
b) methodologies and modes of explanation or interpretation,
c) relationships of researches to a) and b),
d) relationships between research contents (a, b, c) and wider historical contexts (Koerner 2008).

In consequence, there are considerable similarities between paradoxes arising in social anthropology and archaeology. For instance, in both fields, whilst approaches that:

> depart from the nature pole tend to treat each society as a specific homeostatic device tightly adapted to a specific environment… culturalist perspectives see each society as an original incommensurate system of imposing meanings on a natural order, the definition and definition of which are nevertheless derived from western conceptions of nature…. Paradoxically, the purported universalism of geographic determinism thus leads to an extreme relativism, while the self-claimed cultural relativism leaves unquestioned its assumptions of a universalistic conception of nature (Descola and Pálssen 1996: 4).

2.2. Experimental archaeology and the parting of 'New' and post-processual paradigms

Today there is considerable agreement that the 'new' paradigms of the 1970s made major contributions to:

a) technologically sophisticated approaches to experimental archaeology,
b) challenging numerous very problematic generalisations about human behaviour and social evolution,

c) initiating hitherto unimaginably diverse lines of socially oriented research (e.g., Johnson Shanks and Tilley 1987; Trigger 1989; Wylie 1989, 1993, 2002; Meskell and Preucel 2004).

But there has also been continuing attention (including amongst founding figures of New and processual programmes) to three critiques. One is the critique of the idea rooted in logical positivist and logical empiricist philosophical traditions, that methodology and 'middle range theory are independent of 'general theory.' At issue has been concerns that this idea results in failures to take the 'theory-ladenness' of all methodology into adequate account (Trigger 1989; Wylie 1989, 1993, 2000). Second, critics have objected to uniformitarian assumptions. For Johnson (1999, 55), "we have to assume that the conditions in the past were like those in the present. If conditions in the past varied, all bets are off; anything could have happened, actualistic studies in the present offer no secure guide to what might have happened in the past." For many post-processualists, such assumptions result in reducing human behaviour to responses to forces of nature (population growth, the 'carrying capacity' of the environment, and so on), disregarding cultural ideas and ignoring past people's very different 'world views' and/or 'alternative realities'. But perhaps, the most recurrent post-processualist critiques have centred on objections that New and processual approaches to experimental archaeology have aspired to an "analogous path to that of natural sciences" (Johnson 1999: 40).

3. Tenacious Presuppositions

Researchers have responded in a variety of ways. For instance, Richard Bradley (1993, 132) stressed the field becoming divided: "with one faction, who hold onto the methods and aspirations of the scientist, and another who are engaged in fervent introspection and regard that aspiration to scientific method as a political position in itself." For Bradley, "both groups have reached that position from a common starting point – "disenchantment with the archaeological record."

> There is too much pressure from both sides. One group urges us to relate any idea about the past to more general principles of human behavior. These should have test implications, which preferably employ measurement and can be examined for statistical significance…. The other group scrutinized every thought for gender, class, or racial bias, concluding that the main function of studying the past is to criticize the present (Bradley 1993, 132)

Two broad concerns have recently converged:

a) Concerns with 'received models. For instance, writing on problems that dualist characterisations of "biology and culture" and uni-linear models of "human development from simplicity to complexity" pose for appreciating the diversity of the Upper, Middle and Lower Paleolithic, Clive Gamble and Erica Gittens (2008, 105) suggest that "the failing does not lie with each new position that archaeology adopts…. Instead, [it] stems from the inherited intellectual framework of Western thought that predates the existence of archaeology but within which archaeology operates."

b) Concerns that apparently antithetical paradigms impede awareness of alternatives. For instance, for Bruce Trigger (2004, 47) one of the main difficulties with debates over "processual and post-processual" paradigms has been that, while each side claims to be the sole possessor of the true explanation of the archaeological record, they jointly believe that there are no discovered alternatives to the issues they debate. In so doing, they trap themselves in a dichotomy between Enlightenment rationalism and counter-Enlightenment romanticism that has dominated Western social thought since the eighteenth century."

Exploring a number of problematical presuppositions about science and the world, which have been shared by the most polemically opposed positions on experimental archaeology may be highly relevant for fresh approaches to these concerns. In this section we explore in a bit of detail something of the paradoxes of three such presuppositions.

3.1. Presuppositions about the ultimate tasks of science

Throughout the 19th and 20th century (and to this very day), the most polemical debates over whether humanities and social sciences can or should aspire to the requirements of a science have revolved around shared presuppositions that the ultimate task of science is to establish universally valid concepts, categories and propositions, whose explanatory force is independent of any context of application. The roots of these orientations date back to Plato (427–347 BC) (1999), Aristotle (384–322 BC) (1984) and their relativist (or skeptical) critics' models of human agency, history and divisions of the world between: (1) absolute unity and permanence, and (2) absolute dis-unity, pure flux (or in modern terms absolute 'randomness'). Aristotle (1994) created a scheme for framing contrasts between opposing paradigms in terms of a question about what sorts of things satisfy the requirements of a science (episteme), namely: if something can be said to be subject to change, what is the essence of that something? (1) The unchanging aspect, (2) the changing aspect, or (3) both, that is, the interaction of changing and unchanging aspects? For foundationalists (like Socrates and Plato), the answer must be (1), and the others have to be reducible to it. Scientific objects must exhibit regularities that are universal and demonstrable by chain of both necessary and sufficient causes. For probabalists (like Aristotle), things that are "always or for the most part" can satisfy requirements of science if they can be described as examples of essential states or substances (Aristotle, *Metaphysics* 1994, 1027a20–27; cf. Daston 2000). For skeptics – as for "subjectivists" (Bourdieu 1970) and number of strong cultural relativists – the only thing that is unchanging is the illusion of unchanging things – all is by chance and/or caused by such things as social interests and values.

3.2. Presuppositions about the supposed simplicity of the world

One consequence of these presuppositions is that they result in reducing issues of ontology (theories about what sorts of things exist and how they came into being), epistemology (theories about knowledge and especially about obstacles to objectivity) and ethics to matters of classification. Examples include the complex systems of dichotomies around which much debate over processualism and post-processualism has been structured (e.g.,

Augé 1977; Rorty 1979; Descola and Pálssen eds 1996; Koerner 2008). The presupposition at the heart of this tendency is that the most essential property of the world is simplicity (for instance, Boyle [1686] 1996; Einstein, correspondence 1903–1955, 1972). Ever since the mathematical – mechanical science and natural philosophies of Bacon ([1628] 1884, 1974), Boyle ([1686] 1996), Descartes (1984–91), Newton ([1687] 1934), Hobbes ([1651] 1962) became something of the 'philosopher king' of the "modern cosmopolis' (Toulmin 1990) the recurrently predominant ideal vision of the world has been one of reducibility to simplicity.

> The accepted model has been one of geometry; and the stated aim was to enable the control of the whole natural world by routine operations, like those of a mechanic. Ever since then, developments in science have been counted as advances if they further articulated that paradigm of simplicity. Its model has been of individuals conceived rather like Hobbes' atomic persons; and it eventually enabled the exclusion of design and purpose from biological explanations. Descartes' 'beau roman de physique' (as his disillusioned follower Huygens called it) finds its contemporary manifestation in that biological science which finds anything bigger than a cell too complicated to be a worthwhile object of study (Funtowicz and Ravetz 1997, 792).

Importantly, reducibility to simplicity is not a theme restricted to Enlightenment rationalism and objectivist philosophy (e.g., Hempel and Oppenheim 1948; Churchland and Churchland 1998). It is likewise a key theme of the beliefs held by many scientists (as well as specialists in the humanities and social sciences) about the nature of the major obstacles to discovering a supposed determinate order "behind the complexity," including such obstacles as "inadequate technologies and human 'states of mind'" (Stengers and Prigogine 1984). No less remarkably, it is likewise a presupposition that motivates relativist tendencies to equate the historically contingent with the illusory (cf. Ingold 2000). For Daston, what bears stressing is how, in debates over apparently antithetical objectivist and subjectivist paradigms:

> The opposition between nature and culture shadows that between the real and the constructed, nature stands as the eternal, the inexorable, the universal; culture for the variable, the malleable, and the particular. Like the return of the represses, the supra- and sub-lunary spheres of Aristotelian cosmology crop up in a new guise, crystalline nature encircling mutable culture. Both sides of the debate accept the oppositions of the real versus the constructed, the natural versus the constructed. Hence arguments are about which of these two categories notions like 'race' or 'quark' belong – are they real or are they constructed? Discoveries or inventions? – Not about the categories themselves (Daston 2000, 3).

3.3. Presuppositions about relationships between contexts and contents of science

All this might not be so relevant for archaeology and anthropology if nature-culture, moderns – pre-moderns, reality – historical contingency dichotomies did not play essential (and highly paradoxical) roles in presuppositions not only about relationships between contents and social contexts of science, but about the contents of the very 'hardest' of sciences (such as physics and some neo-Darwinian paradigms for molecular biology). Here, we explore only two elements of this situation. As noted at the onset, both essentialists and relativists are concerned with relationships between the contents

and social contexts of science. The former stress "doing boundary work" to separate the two, the latter "watch boundaries" and argue that the contents of scientific knowledge is reducible to social values and interests (cf. Gieryn 1997). Both eclipse at least two things:

a) the importance of dualist characterisations of nature-culture and images of science and modernity to the distinctions 'foundational laws' and 'phenomenological generalisations',
b) connections between (a) and problematical images of called 'modern and other world views'.

3.3.1. Problematic Divisions of 'Foundational Laws' and 'Phenomenological Generalisations'

For several decades, Ilya Prigogine, Isabelle Stengers, Latour and quite a number of others have been exploring the roles in mainstream experimental and theoretical science of distinctions between 'phenomenological laws' and 'fundamental laws,' and views that although the former can describe phenomena mathematically in a rigorous and relevant way, only the latter can claim to unify the diversity of phenomena – that is, to go '**beyond appearances**'. Of special relevance for our present concerns, are the impacts this distinction has on treatments of temporality, irreversible change and the emergence and endurance of relationships and entities in states far from equilibrium. Stengers (1997, 22–23) writes that when she began studying physics, she accepted the mainstream belief that observations on irreversible processes (mixtures that do not un-mix, radical differences between before and after, where it is impossible to overlook the non-equivalence of cause and effect) should be treated "merely phenomenological' consequences of our not being "perfect observers" and inadequate instruments. Working with Prigogine, she began to explore the extent to which the history of paradigms for the tasks of science that deny time (and irreversible change and emergent novelty) has also been the "history of social and cultural tensions" (Stengers 1997, 42–43).

In the humanities and social sciences (and in debates over influentially opposed positions on experimental archaeology) much disagreement centres on qualitative and quantitative methods. One problem with directions these debates have taken is the tendency to eclipse paradoxical consequences of the roles assigned to the equals sign (=), including that of erasing differences between before and after, and cause and effect. Stengers (1997, 22–23) explains that, one the one hand, we are supposed to regard distinctions between 'phenomenological observations' and 'fundamental laws' (laws of changeless change) as crucial. Then we are supposed to use the 'equals sign' to address the supposed cause of these distinctions, namely, supposed 'mere appearances' and instruments. What bears stressing, for Stengers (1997, 22–23), is that, instead of accepting that using the 'equals sign' to erase before and after is theoretically incoherent and empirically refutable, some go on to "judge the world of phenomena in the name of a normative ideal… more virulently than ever."

Of course, notions of cause, effect and an 'equals sign' are of extraordinary antiquity. What is new is the idea that one can treat a present state as an effect equal (and reversible) to both a past state and an identical future. Values that have been invested in computer technologies (including values that attribute these technologies

with supposed capacities for 'a-perspectival objectivity') (Daston and Galison 2007) have helped facilitate this problematic idea. Another aspect of the history of this idea, which bears stressing, has been applications of problematic interpretations Charles Darwin's notion of the randomness of natural selection to realms studied by physicists and, most recently, by a number of molecular biologists (Prigogine 1997). In several 20th century deterministic interpretations of absolute randomness, all processes end up being treated as though they were time-reversible – that is, as though they can proceed backward as well as forward through time. For Prigogine (1997), determinism is paradoxically, both a way of envisaging trajectory and a fundamental denial of what Arthur Eddington (1958 [1927]) called "the arrow of time." One of the consequences of the paradox is that it prohibits common sense experience of change. Determinism prohibits appreciation of an "event in the present" arising in relation to a "past that precedes an undetermined future" (Prigogine 1997). It treats time as a paradoxically timeless 'given' and the future as arising – not from a plurality of indeterminate initial conditions – but as if it was as determined as the past.

The importance of dualist characterisations of nature versus culture and of the real versus the historically contingent to the ways in which the above outlined conceptions of 'foundational laws' versus 'phenomenological generalisations' lead to determinism is difficult to overstate. There may quite a number of ways to illustrate this theme. Our concerns in this chapter make it useful to depart from the notion of 'elimination'. Elimination and reduction are expressions developed by scientists (and analytic philosophers) to talk about objectivist (natural or social) theories about knowledge and explanation (Dennett 1978; Rey 1988; Churchland and Churchland 1998). But recent efforts to 'go beyond' nature–culture and related dichotomies indicate there are subjectivist and cultural relativist versions too (cf. Bourdieu 1970; Latour 1993, 2004; Descola and Pálssen eds 1996; Latour and Weibel 2002, 2005). Despite claims to the contrary, in both some of the most influential objectivist and subjectivist views, everyday people's common-sense understandings of the world, the mind, and so on are 'folk psychology' and reducible to supposedly more fundamental substances, like 'the environment,' 'society,' 'collective representations,' and so on. Further – and this cannot be boldly underscored enough - such caricatures of 'folk psychology' (like those of 'pre-modern world views') have played problematical roles in shaping caricatures of 'phenomenological generalisations' as 'mere appearances'.

3.3.2. Contradictory Aspects of 'Standard Accounts' of Science and Modernity

Things would not be so complicated if the above outlined problems were not so deeply enmeshed in further contradictions, including contradictions between the above outlined presuppositions about the tasks of science and the supposed simplicity of the world, and about what distinguishes 'scientific' and/or modern' from 'other worldviews' and/or 'folk psychology'. Despite claims to the contrary, new versions of the latter grounded in dualist characterisations of 'reason versus tradition' (Ingold 1993, 2000), and especially the idea that what distinguishes modern reason and science form all 'other' forms of thought and practice is it awareness of the contingency of all human claims to truth (cf. Blumenberg 1983; Funkenstein 1986). In so-called 'standard accounts' of science and modernity (whether interpreted as triumph or tragedy), once

this awareness had been established science could be expanded but it could not recover (nor did it wish to return to) the Aristotelian world picture. In such accounts, the most fundamental difference of 'other world views' are the ways in which their being situated **in traditions** (Ingold 1993) supposedly impedes and/or denies awareness of contingency, the passage of time, even history (Blumenberg 1983; J. L. Koerner 1998).

But how can this be? Would a model of a world view, which supposedly denies contingency, not fit better the deterministic presuppositions about the task of science and simplicity of the world that we have been exploring? Such questions have led Stengers (1997) to argue that complexity is not something that we have to "discover." Like awareness of there having always been alternatives to vexed options of objectivism and subjectivism, it is something that we somehow always knew all along. Stengers' approach to the problem departs from observations that:

> we get used to associating the birth of modernity with the 'Copernican revolution,' the substitution of the heliocentric system for the geocentric system. But it was Kepler who made the real difference between the two systems by transforming the significance of the relation between mathematics and astronomy." Kepler divested the circle, and with it, mathematics and geometry of the powers they had been hitherto assigned of 'sufficient reason' – and the status they held as a priori timeless placeless judge of epistemic authority and ascribed political sovereignty ascription (Stengers 1997, 20–22).

What bears stressing is that Kepler's 'breaking the circle of sufficient reason' played essential roles in making explicit the extent to which Bacon's experimental science and Newton's natural philosophy undermined the presuppositions of scholars since antiquity about distinctions between the essential substances of sublunary and celestial realms, and their implications for ontic sources of objectivity. Thus, we should expect the founding figures of early modern experimental science and natural philosophy to have wanted to extend properties hitherto attributed to the contingency of sublunary realms to the ends of the universe. And several argued precisely such aims (for instance, Galileo [1564–1642] 1968). But one of the remarkable things about the presuppositions about the supposed context independent tasks of science, which we considered above, is that their notions of the supposed simplicity of the world (and of time reversible 'foundational laws') do not resemble historically contingent sublunary realms. To the contrary, they resemble the Aristotelian **celestial sphere** – "the unchanging and divine world of astronomic trajectories, which was for Aristotle the only world that could be given exact mathematical and geometric description" (Stengers 1997, 34–35). Or put another way, they share features with caricatures of world views of so called 'pre-moderns,' 'others' and 'folk psychology.'

4. Recent Developments and New Directions in Experimental Archaeology

This section concerns developments, which go against the grain of conceptions of 'the experiment,' which challenge the presuppositions we explored in Part 3. In order to bring the novelty and relevance of these developments into relief, our considerations are

structured around three challenges stressed by editors of *Nature and Society. Anthropological Perspectives* (1996).

4.1. Not Just another Analytic Category

> Deconstructing the dualist paradigm may appear as just one more example of the healthy self-criticism which now permeates anthropological theory. After all, burning conceptual fetishes has long been a favorite pastime of anthropologists and very few domains have escaped this iconoclastic trend. If such analytic categories as economics, totemism, kinship, politics individualism, or even society, have been characterized as ethnocentric constructs, why should it be any different with the disjuncture between nature and society? The answer is that this dichotomy is not just another analytic category belonging to the intellectual tool kit of the social sciences: it is the key foundation of the modernist epistemology (Descola and Pálssen 1996, 12).

Until rather recently, the notion of 'the experiment' as a somehow universally valid, context independent means to predict and control contingency has figured paradoxically amongst both the **most** and the **least** historicised of all themes in both so-called 'standard accounts' of the Scientific Revolution and Birth of Modernity and in many critiques, which envisage science and modernity as a tragedy, rather than as a triumph. The expression 'most and least historicised' is very useful for illuminating what have been quite widespread problematic trends. On the one hand, the notion has figured amongst the **most** historicised themes relating to the remarkable roles it has been given both in Enlightenment and counter Enlightenment romantic accounts of factors, which have been said to have been most responsible for separations of modernity both from its pre-modern past and the histories of all 'others.' For Weber, the "first great achievement" in western thought was Plato's idea of "the concept," and the second was Bacon's scheme for testing concepts under "experimental conditions." In Bacon's "system" for a *Novum Organum* (1885, [1620] 1974), the primary task of experimental science and mathematical-mechanical natural philosophy was the pursuit of certainty through technologically controlled interventions. "Experimental conditions," in this view, were believed to reveal the immutables of Nature (in its "course and constancy") behind contingent manifestations of Nature's "erring or varying" and being "altered or wrought, modified by the presence of man" (Bacon 1974).

On the other hand, 'the experiment' has figured amongst the **least** historicised themes relating to problems, such as disregard of questions about the particular historical circumstances under which it came to play pivotal roles in meta-narratives about science, modernity and the difference of so-called 'others' went re treated though they as self-evident. Little attention was given to the concrete historical circumstances under which 'the experiment' came to be envisaged, for instance, as marking divisions between 'modern and pre-moderns' and 'reason and tradition,' and even as restructuring the very constitution of cosmos and history.

It bears stressing that at issue with the "neglect of the experiments" during much of the 20th century, few authors mean that "scientists were neglecting experiments; spinning well financed cobwebs of theories while laboratories decayed for lack of funds" (Hacking 1989, 147). The problem was more that historians, philosophers, and scientists who contributing to the historiography of their fields tended to neglect

context dependent dimensions of science, in general. Much attention focused, instead, on such supposedly context independent themes as those "of the theory/observation distinction, or the impossibility of eliminating theory by crucial experiment, etc…. The even-handed Dictionary of Scientific Biography cut articles on experimenters ["and there were remarks…, *a propo* of that 'tedious recounting of test tubes and jottings' "] while expanding articles on theory" (Hacking 1989, 147).

Such problems led John Hale (1993) to explore in great depth the historical circumstances under which the 'new experimental science' and 'mechanical natural philosophy' became pedagogical and political ideals of what Toulmin (1990) refers to as the "modern cosmopolis" (Toulmin 1990). Writing on the remarkable circumstances of the "discovery (and/or "invention") of Europe," Hale (1993) noted that one of the most striking features of Bacon's hopes for the historical significance of experimental science and mechanical philosophy was the expression "we Europeans." Amongst other things, the expression assumed that "his readers knew where 'Europeans' were, who they were, and what, in spite of national differences, they shared" (Hale 1993, 1). Such assumptions could not have been made under circumstances that differed to those Bacon described. In Bacon's view, the "new system" he developed for the sciences he developed was necessary not only to bring an end to what would be called the Thirty Years War (1618–1648), it was of fundamental cosmological significance. The 'common world' forged by the 'new system,' for Bacon, would replace the "idols of tradition" with "Reason" and even reverse consequences of the Fall, which he and many of his powerful contemporaries believe to pose the greatest threats to social order (Schaffer 2002). What bear underscoring for our present purposes, is that the connotations of the expression, "Europeans," and historical significance of such expectations are not self evident but require contextually situated investigation.

The expression "the **most** and the **least** historicised" comes from an article entitled The History of Science as Self Portraiture" that Lorrain Daston wrote on the occasion of the award of the 2005 Erasmus Prize for work in the history and philosophy of science to Simon Shapin and Steven Schaffer, especially for their jointly authored study, *Leviathan and the Vacuum Pump Hobbes, Boyle and the Experimental Life* (1985). For Daston, the work marked an epoch of change in the kinds of questions historians posed and the ways in which they went about answering them:

> Perhaps the simplest ways of describing the change is that what had previously been regarded as self -evident now demanded historical explanation: "What is an experiment? We want our answers to be historical in character [Shapin and Schaffer 1985: 3]. Moreover, the historical explanation in question linked fundamental innovations in science such as the emergence of the experiment as a method of inquiry to coeval political and social events (Daston 2006, 523).

Despite the remarkable variety amongst approaches, we can discern several shifts in orientations that have made especially important contributions to appreciating the remarkable "historiality" (to borrow Rheinberger's 1996 term) of the 'experiment' and cross-culturally open-ended approaches to 'experimentality' (Koerner and Szersynski 2009). Amongst others, these include three broad shifts in foci.

– from notions of science, rationality, experiments and objectivity as "givens" ("natural kinds") towards inquiries into the variety of forms such concepts have taken

Few developments have contributed more significantly to radical change in orientations towards highly problematical dualist categories than inquiries that have moved the centre of research foci from the levels of abstraction at which much intellectual history operated to concrete circumstances, practices and entities. Importantly, this did not involve abandoning abstractions but the development of methodologies for investigating their coming into being and historical significance in concrete terms. Thus, for example, instead of asking whether Bacon's conception of an experiment accords with Weber's, researchers began to inquire into the entities and associated matters of social concern, which Bacon, Newton and others were engaged with. Daston refers to such approaches as 'applied meta- physics' and has written extensively on:

> how whole domains of phenomena – dreams, atoms, monsters, culture, morality, centers of gravity, value, cytoplasmic particles, the self, tuberculosis – come into being and pass away as objects of scientific inquiry. The echo to the title of Aristotle's treatise On Generation and Corruption is deliberate: this is a meta-physics of change, of the "perpetuity of coming-to-be." If pure meta-physics treats the ethereal world of what is always and everywhere from a God's-eye viewpoint, then applied meta-physics studies the dynamic world of what emerges and disappears from the horizon of working scientists (Daston 2000, 1).

– from intellectual history (the history of ideas) towards practices, instruments, cycles of credibility

'Applied meta-physics' has contributed to major change in methodological orientations. Instead of having to chose between approaches to intellectual history (the history of ideas) and approaches to social contexts (or between macro- and micro-analytic scales), applied metaphysics concerns the contextual circumstances (practices, instruments, cycles of credibility, social relations, and so on) under which "epistemic things or entities" ("experimental objects and systems") emerge, are transformed and/or endure in states far from equilibrium (Rheinberger 1997). For Hans-Joerg Rheinberger (1997), one of the most interesting things about such entities (contrary to notions that 'the experiment' is universally concerned with eliminating contingency) is precisely their lack of determinate completeness. They are objects of inquiry, as well as open-ended sites where further novel objects, instruments and practices emerge. Instead of being closed, experimental objects are open to question, controversy and projection. Instead of owing their significance to reduction (as the Occam Razer story about the simplicity of the world goes), the significance of "experimental things" (or 'matters of concern') hinges upon **adding** to their complexity – and to their implications for widening the diversity of possible futures (see also, Callon 1986; Latour 2008).

The terms **representing** and **intervening** were introduced by Ian Hacking (1983) in order to shift focus away from traditions centering on abstract ideas to inquiries into experimental instruments and practices. Notions of representation, for Hacking, perpetuate the realist/antirealist debate. For Hacking, much of the tenacity of debates over foundationalism and relativism has been a consequence of ignoring the jointly epistemic and political dimensions of representations. By exploring practices of

representing and of intervening in physical, organic and social realms, we can appreciate the concrete historiality of the instruments, activities and relationships whereby we intervene in the world, which shapes how our surrounding impact us. In this view, the primary aim of experimental practice is not to create representations but to change the world and especially our social relationships with it. As Latour and Steve Woolgar (1979) put it, we don't formulate our representations of objects of knowledge production first, and then start to think about audience **persuasion** afterwards. Concerns with persuasion are always there from the start, over evidence, adequacy, salience, consistency, and especially over audience's capacities (or lack thereof) to believe what we supposedly know is true.

– FROM PRESUPPOSITIONS THAT CONSENSUS IS DETERMINED BY NECESSITY TOWARDS CONCERNS WITH HOW CONSENSUS IS ACHIEVED, IS TRANSFORMED AND ENDURES

By treating objects of experimental practice – as well as such epistemic things as nature-culture, reality-tradition – as "simultaneously real, historical [and contingent upon] political and social events," applied metaphysics contributed to radical changes in orientations towards so-called 'standard accounts' of science and modernity. Amongst other things, these changes include remarkable shifts away from presuppositions that consensus and continuity are somehow determined by necessity towards questions about how consensus (a 'common world') is created. They have also encouraged replacing the long history of preoccupations with 'crises over representation' by inquiries into materials, which bear directly upon problems created by the importance of highly generalised 'standard accounts' or 'meta-narratives' about science and modernity (and associated nature-culture dichotomies) to Western epistemology, in general, and archaeology and anthropology's most influentially opposed theoretical paradigms (Descola and Pálssen 1996, 12; Trigger 2008). 'Standard accounts' of science and modernity conventionally stress 17th century economic prosperity, the withering of religious restrictions on social mobility and intellectual life, expansion of secular culture, the political centrality of the nation-state, and the overturning of pre-modern worldviews by the new mechanical experimental science and natural philosophy (Toulmin 1990; Koerner 2004). But the deeper we delve into materials eclipsed by influentially opposed abstract interpretations of modernity as either a triumph or as a tragedy, the more we become able to understand the complex historical circumstances under which highly problematical caricatures of 'others' came to figure centrally amongst means to supposedly settle 'crises over representation' around presuppositions that we considered in Part 3. Writing on the contextual circumstances under which Bacon argued for the importance of 'the experiment' for social order, Toulmin (1990) describes (not prosperity and well being) – but the worsening conditions that dragged the Thirty Years War to an end:

> The longer the bloodshed continued, the more paradoxical the state of Europe became. … For many of those involved, it ceased to be crucial what their theological beliefs were, or where they were rooted in experience, as 16th-century theologians would have demand. All that mattered, by this stage, was for supporters of Religious Truth to **believe, devoutly in belief itself**. For them, as for Tertullian long ago, the difficulty of squaring a doctrine with experience was just one more reason for accepting this doctrine that much more strongly (Toulmin 1990, 54).

What bears stressing, is that it was not until peace negotiators acquired such devout **belief in the beliefs** of 'others' that it became possible to supposedly settle 'crises' in ways that (a) reduce existential and moral crises to problems of expert competence, (b) centre on the 'myth of the clean slate' (Toulmin 1990), (c) eclipse the importance of acknowledging disagreement to anything like a 'common good'.

4.2. New Alliances between the History of Humankind, Societies, and Nature's Exploratory Adventure

> It is realistic to assume that the environment matters and that to understand both humanity and the rest of the world anthropology, ecology and biology need new kinds of models, perspectives and metaphors. Such a realization may necessitate a fundamentally revised division of academic labour; in particular, the removal of disciplinary boundaries between the natural and the social sciences. We may have to abandon the current separation of physical and biological anthropology, giving new life to the old philosophical, anthropological project which focused on the unity of the human being (Descola and Pálssen 1996, 14).

To the best of my knowledge, few anthropologists and archaeologists have made more important contributions to interest in the relevance of new interdisciplinary alliances for appreciating not only that 'we have never been modern' in the ways standard accounts claim, but also that no one has been or will be 'pre-modern' than John Barrett (1994, 2000) and Tim Ingold (1993, 2000). Barrett long argued for the relevance of the indeterminacy of human life ways and histories for fresh approaches to problems with influentially opposed "physical and textual models of the archaeological record" (Patrick [1985] 1999). For Barrett such approaches might explore "the range of contextual mechanism by which different forms of agency have gained their various historical realities" (Barrett 2000, 61). Human agency, Barrett says:

> … is not something which lies behind the material residues of an 'archaeological record', to be recovered (literally 'dug out') by the archaeologist who explains that 'record' as being a 'record' of something' ('ideas', 'actions', etc.). Instead, agency lies in front of [its historical and material conditions as]...the means of understanding and reworking them in an interpretive cycle, and it is this interpretive cycle which can be glimpsed through archaeological analysis. [And an] archaeological engagement with the past now becomes an attempt to understand how, under given historical and material conditions, it may have been possible to speak and act in certain ways and not in others, and by so doing to have carried certain programmes of knowledge and expectation forward in time (Barrett 1994, 4–5).

Interestingly, with such orientations, we come close to the conception of time developed by Prigogine, and suggested by works of Ingold (2000), as well as several contributions to the present volume, which envisages time, as an operator (a property of dynamic relationships) rather than as an external parameter. For Rheinberger (1997, 180), what is especially interesting about the idea of operator time (or internal time) is that it can help illuminate indeterminate dynamic relationships analogically (see also, Kubler 1962).

> Let us assume that with respect to the movement of material systems, systems of things, or systems of actions, time can be viewed as an operator abnd not simply as a chronological axis of extension in a system of coordinated. In this sense, time is structural, local and

> intrinsic characteristic of any system maintaining itself in at least some respects enduring state – though far from equilibrium and reaching from time to time, as a result of turbulences, points of bifurcation…. Thus every system of material entities, and therefore any system of actions of such entities can be said to possess its own intrinsic time a process" (Rheinberger 1997, 180).

Unfortunately, the implications of these arguments for appreciating the relevance of new alliances between sciences of nature and humanity for 'going beyond' dualist caricatures of 'others' has taken some time to recognise. Few have played more important roles in changing the situation than Ingold. Ingold has never ceased to examine complex historical connections between problematic disciplinary divisions and caricatures of 'others'. By so doing, he has thrown extraordinary light on connections between schemes that divide the biological evolution of 'anatomically modern humans' from cultural history, and problems with hitherto predominant conceptions of 'cross-cultural translation' of supposedly 'incommensurable world views'. The former hinge upon the complex presuppositions that motivate models of how humanity supposedly stepped out of 'nature' through the emergence of "capacity to reason," and of how science and modernity supposedly "represents the culmination of the potentials common to humanity" (Ingold 1993, 215–117). Both depend on dualist characterisations of nature versus culture, "universal capacities to reason versus tradition" and "the real and total versus the local and historical" (Ingold 2008). Furthermore, in both interpretations of modernity as a triumph and as a tragedy, the emergence of experimental science and mechanical philosophy marked a break in cultural history, perhaps of no lesser significance than the break between nature and culture. In both options, traditional "views in the world" are replaced by the supposed universal "view of the world" that is alone capable of disclosing the 'others' "as world views, as alternative ('emic') modeling" of a supposedly immutable independent ('etic') reality" (Ingold 1993, 224). Much of Ingold's recent work centres on critical and constructive approaches to interlinked problems with:

a) deterministic approaches to molecular biology that eclipse the importance of emergent novelty to processes of growth and maturation, and the diversity of life forms and their relational capacities (see also, Gould and Lewontin 1979; Jablonka and Lamb 1995; Jablonka 2000),
b) approaches to cognitive psychology, which depart from dualist characterisations of the mind versus the world (see also, Pribram 1997),
c) versions of cultural theory that attribute human behavior to designs that are passed from one generation to the next as the content of traditions (Ingold 1996, 2000; see also, Gamble 2008).

This work has led to constructive hypotheses that are likely to be especially relevant for strongly reflexive experimental archaeologies, including that:

a) Much of what we call cultural variation may consist of "variations of skills – not just techniques of the body, but capabilities of action and perception of the whole organic being."

b) "Becoming skilled in the practice of a particular form of life is not a matter of furnishing a set of generalised capacities, given from the start as compartments of universal human nature, with specific cultural content." Rather than being transmitted from generation to generation by tradition, they are "re-grown, incorporated into the modus operandi" of the developing human organism person through training and experience it the performance of particular tasks.
c) "One of the mistakes of anthropology has been to insist upon a separation between the domains of technical and social activities, a separation that has blinded us to the fact that one of the outstanding features of human technical practices lies in their embeddedness in the current of sociality. It is to the entire ensemble of tasks, in their mutual interlocking, that I refer by the concept of taskscape (Ingold 2000: 6, 298).

4.3. Complexity and Reflexivity

> Going beyond dualism opens up an entirely different landscape, one in which states and substances are replaced by processes and relations; the main question is not any more how to objectify closed systems, but how to account for the diversity of the processes of objectification (Descola and Pálssen 1996, 12).

For Ingold, as for several contributions to the present volume inspired by his insights, strongly reflexive experimental archaeologies centering on skilled practice are likely to bear very directly upon challenges suggested by the passage above. In Ingold's work:

a) Intentionality and functionality are immanent in the practice itself rather than being prior properties, respectively of an agent and an instrument.
b) Skill is not an attribute of the individual body in isolation but of the whole system of relationships constituted by the presence of the artisan in his or her environment.
c) Rather than representing the mere application of a mechanical force, skill involves care, judgment and dexterity.
d) It is not the transmission of formulae that skills are passed from generation to generation, but through practical, 'hands on' experience.
e) Skilled workmanship serves not to execute a pre-exiting design, but actually to generate the forms of artifacts (Ingold 2000, 291; see also, Ingold 1996).

Complexity and reflexivity are key components of Ingold's (2000, 5) approach to "the human being as a locus of creative growth within unfolding fields of relationships." As such human livelihood, sociability, communities and histories are loci of creativity that participate in interacting rhythms. For Elizabeth Hallem and Ingold (2007, 2–3) inquiries into such loci of creativity can help to challenge – rather than perpetuate such dichotomies as those of nature-culture, the global versus the local, the polarity between novelty and convention, or between the innovative dynamic of the present and the traditionalism of the past, that has long formed such a powerful undercurrent to the discourses of modernity." Instead of "innovation," they stress the usefulness of the notion of improvisation, explaining that:

> The difference between improvisation and innovation …is not that the one works within established convention while the other breaks with it, but rthgat the former characterises creativity by way of processes, the latter by way of its products, To read creativity as innovation is, if you will, to read it backwards, in terms of its results, instead of forwards, in terms of the movements that gave rise to them (Ingold and Hallem 2007, 2–3).

In this view, like skills, improvisation is not about the already made, finished and given. Amongst other things, it is:

a) generative – "rather than the realisation of an *a priori* design,"
b) temporal – inherent in the "onward propulsion of life rather than being broken off, as a new present, from a past that is over,"
c) relational – "the creativity of our imaginative reflections is inseparable from our performative engagement" with the dynamics of our surroundings (Ingold and Hallem 2007, 3).

Envisaging human livelihoods and temporalities of landscape along these lines is intrinsically reflexivity: "it situates the practitioner from the start in the context of active engagement with his or her surroundings" (Ingold 2000, 5). Further, reflexive orientations towards the temporalities of landscape are not restricted to human or even only to animate realms. For Ingold:

> The rhythms of human activities resonate not only with those of other living things but also with a host of other rhythmic phenomena – the cycles of the day and night and of the seasons, the winds and the tides, and so on…. [L]ife is not a principle that is separately installed inside individual organisms, and which sets them in motion upon a stage of the inanimate. To the contrary…life is a name for what is going on in the generative field within which organic forms are located and 'held in place' [Ingold 1990, 215]…. This means that in dwelling in the world, we do not act upon it, or do things to it; we move along with it. Our actions do not transform the world; they are part and parcel of the world's transforming itself. And this is just another way of saying that they belong to time (Ingold 2000, 200).

Similarly, writing on changing approaches to the indeterminacy of the "metamorphosis of nature," Stengers recalls Michel Serres evoking the respect that peasants and fishermen have for the world in which they live:

> They know that no one has control over time and that one cannot rush the growth of the living, the autonomous transformations that the Greeks called physis. In this sense, science may be at last on its way to becoming a physical science since it has to finally accept the autonomy of things, and not just living things…. As with the development of plants, the development of this new nature, peopled by machines and technology,, the development of social and cultural practices, the growth of cities are continuous and autonomous processes in which no one can certainly intervene to modify or organize them, but whose intrinsic time must be taken into account, under threat of failure (Stengers 1997)

In these lights, she argues that the "time has come for new alliances, which have always existed but for a long time have been ignored between the history of humankind, its societies, its knowledges, and the exploratory adventure of nature" (Stengers 1997, 59).

5. Some Implications for Strongly Reflective Experimental Archaeologies

We began with observations that until quite recently researchers are likely to have been receptive to suggestions that experimental archaeology could provide very useful contexts for exploring the relevance of insights that follow from appreciating that a 'common world' is not given for 'going beyond' for efforts to 'go beyond' such dichotomies as those of nature-culture, science-values, moderns – pre-moderns, reason versus tradition, and, especially, the "global and universal versus the local and illusory" (cf. Rorty 1979; Ingold 1993, 2000). Today the situation is very much changed. In tandem with the developments, which we considered in Part 4, there has been remarkable growth in awareness that familiarity with disputes in academic settings over essentialism and relativism do not offer much help in situations where - despite claims to the contrary – new dualist images that (to use Johannes Fabian's 1983 extraordinary phrase) "deny the coevalness" of the global and the local proliferate in tandem with deepening clashes, for instance, between:

a) processes of "globalisation and risk society" (Harvey 1989; Giddens 1990; Beck 1992; Hardt and Negri 2000; Inda and Rosaldo 2002),
b) new versions of "two cultures" (Snow 1959) – one centring on the increasing normative status of cultural relativism and/or post-modern theory (cf. Galison 1996; Hall 2005) in academic contexts; the other arising out of radical changes taking place in the scales, normative roles and conceptions of sources of the objectivity of science (Kaku 1997; see also, Galison 1987, 2002, 2007; Galison and Hevly 1992; Daston and Galison 2007; Felt and Wynne 2008; Koerner 2008),
c) anthropologists and archaeologists' emphasis on the contextually embedded nature of ethics; and the policy authority of technologies (and expert agencies), which some envisage as able to adjudicate ethical issues in context independent terms (Hastrup 1993; Jasanoff 1995; Strathern 2000; Habermas 1979, 2003; Flyvbjerg 2002; Nowotny 2000; Nowotny *et al*. 2001; Wylie 2002; Meskell 2002; Palang and Fry 2003; Meskell and Pels 2005; Hall 2005; Wynne 2006; Koerner and Wynne 2008).

Few – if any – areas of specialisation in archaeology are likely to be more deeply affected by these clashes than those summarised by the expression "lived cultural heritage". This is especially the case in contexts where controversies are enmeshed in deepening inequalities with regards to exposure to techno-science hazard, unsustainable development and political conflict (e.g. Friedman 2001). Writing on challenges posed by these contexts, in his contribution to an edited volume on *A Future of Archaeology* (Layton *et al*. 2006), Henry Cleere (2006, 65) notes that for many researchers, the UNESCO World Heritage Convention represents both "the best and the worst of contemporary perceptions of and approaches to the tangible and intangible legacy of countless generations of ancestors. One of the most serious charges levelled against the Convention (or those who implement it) is the way in which it is often seen to ride rough shod over the rights and aspirations of local and indigenous communities." Different categories of World Heritage have different implications for the question

"management for and by whom" (Cleere 2006). This is especially the case for the three main categories of "cultural landscapes":

a) "the most easily identifiable is the clearly defined landscape designed and created" as such intentionally, such as "garden and parkland landscapes,"
b) "the organically evolved landscape" that "results from an initial social, economic, administrative and/or religious imperative and has developed its present form by association with and in response to its natural environment," the two major sub-categories of these are: "a relic or fossil landscape) in which an evolutionary process came to an end at some time it the past" but distinguishing features have remained, and "a continuing landscape" that has an active role in contemporary society closely associated with the traditional way of life, and in which the evolutionary process is still in progress,"
c) "the associative cultural landscape" frequently defined in terms of "the powerful religious, artistic or cultural associations of the natural element rather than material or cultural evidence, which may be insignificant or even absent" (Cleere 2006, 68–69).

Based on working more than a decade in World Heritage policy processes, Cleere (2006, 69) notes that it has been category (2) and, especially those where the evolutionary process are currently underway, that pose the most difficult challenges with regards to questions of "management by and for whom"? For Prigogine, Stengers as well as a number of archaeologists working in situations where controversies over the question of "for and by whom lived heritage is being managed" (Cleere 2006) are enmeshed complex jointly social and ecological problems, 'going beyond' dichotomies of nature-culture, science-values, moderns – pre-moderns, reason versus tradition, and the "global and universal versus the local and illusory" (and the presuppositions that we considered in Part 3) is not at all only an academic issue.

To the contrary it has powerful implications for critically and constructively engaging force relations which have produced the differences between those who have got the means to intervene in history and those whose histories needs and aspirations are eclipsed (Stengers 2005; Lazarri 2008). Importantly too, numerous current efforts to integrate strongly reflexive experimental archaeologies into projects to sustain and enhance plurality of 'lived cultural heritage' are grounded in the sorts of insights we have been considering, which follow from appreciating that "common world is not something we can come to recognize, as though it had always been here … A common world, if there is going to be one, is something we have to build, tooth and nail together (Latour 2004, 455).

A useful way to conclude is by mentioning two examples. One excellent example is the project pursued under the direction of James Wescoat (2008) at the newly designated World Heritage site of Champaner-Pavagodh in north-west India (see also, Wescoat 1992). The project shows that landscape archaeology can offer an entirely new approach to heritage, one which "effectively integrates curated objects, protected places, living traditions, and collective memory" by:

a) exploring not only monuments and sites but the region's rich topographic sense of place,
b) emphasing the plurality of temporalities shaping human-environmental relationships (cf. Ingold 2008), especially by encompassing multiple geographical and social scales of historical layers,
c) expressing diverse people – place connections (Wescoat 2008).

Whilst Wescoat lists the range of these conflicts from the **least** to the **most** tractable at the beginning of the chapter, he later lists possible contexts for conciliation in the reverse order:

a) conflict amongst professional agencies (or "experts"),
b) conflict resolution amongst community stakeholders,
c) harmonisation of heritage conservation and economic interests,
d) prevention of intentional destruction,
e) places of violence and sanctuary, as heritage.

Much of the success of the project has hinged upon its emphasis on the experimentality (in the sense of indeterminacy) of all efforts to address these social conflicts, and on upstream participation, on the part of publics with very diverse cultural backgrounds and social experiences, in deliberating the range of social and ecological ends pursued and accountabilities in re-designing the region they occupy together. Amongst these, especially significant have been aims to:

a) harmonise tourist and pilgrim interests,
b) illuminate the manifold historical contributions of Sultanate, Rajput, Jain and tribal groups to the extraordinary complexity of the region's history,
c) deepen contemporary appreciation of the pluralistic legacy at Champaner-Pavagadh (Wescoat 2008).

Such orientations are shared by another group of major projects where controversies over for and by whom heritage is managed are enmeshed in long histories of social and ecological problems. For several decades now, together with colleagues with diverse disciplinary backgrounds and local communities in the Lake Titicaca Basin (Bolivia and Peru), Clarke Erickson (1989, 2006) has carried out experiments in reconstruction raise field systems, "which supported hundreds of generations of farmers" but which did not survive in the area into historical times.

> Excavation indicates that the prehistoric abandonment of the raised fields was due to socio-political changes rather than environmental limitations or change. This implies that, with proper consideration of the contemporary socio-economic context and the sound prehistoric models raised field production has potential for the future development in the Lake Titicaca Basin, [with advantages over] the various capital intensive agricultural schemes being introduces by development agencies… that would destroy the remains of thousands of potentially recuperable raised fields (Erickson and Chandler 1989: 243).

The projects have not at all been motivated by blind confidence, but rather (to borrow Stengers 2005, 998 terms in her contributions to a volume entitled *Making things Public*) (Latour and Weibel eds 2005) by "building up of an active memory" of what Ingold

(2000) would describe as aspects of skill and creativity. Further, just as Ingold stressed the intrinsic reflexivity of such endeavors, much of the success of Erickson's projects have hinged upon agreements, which do not involve supposedly external arbiters. As was the case for the north-west India where Wescoat works, the Lake Titicaca Basin has experienced a long history of external arbiters of all sorts. The former is an extremely difficult environment for present day farmers. Many of the development projects introduced by external development agencies are:

> …. ecologically unsound, relying on introducing crops and farming practices developed for completely different environmental zones. More importantly, these systems are socially and economically inappropriate, relying on heavy capital inputs such as mechanization, petro-chemicals and imported seed. Even if successful, these projects would benefit only a small portion of the population (Erickson and Chandler 1989, 245–246).

By contract the results of archaeological and experimental investigation involving attention to the most tractable problems where possible on the part of local communities and Erickson and his colleagues indicate that raised filed agriculture is:

a) ecologically sound, as well as highly productive and sustainable,
b) socio-economically appropriate (Erickson 1989, 245),
c) respectful of traditional farmers justifiable reluctance to adopt techniques that can and have resulted in social and ecological fiascos (Erickson 1989, 241)

The notions of experimental archaeologies suggested Wescoat and Erickson's projects differ, in fundamental respects, from connotations associated with the idea of 'the experiment' as a somehow providing universal 'laboratory conditions.' Wescoat and Erickson's critically engage the presuppositions about science and the supposed simplicity of the world, which we considered in Part 3. No less important, is how their features go against the grain of the sorts of supposed settlements of 'crises over representation' we considered in Part 4. Examples include such features of Wescoat and Erickson's projects as their:

a) attention to the embeddedness of the historical complexity of sites in jointly social and ecological problems,
b) appreciation of the relevance of context dependent approaches to this complexity for beginning with the most rather than the least tractable problems,
c) insight that it is never too early to include striving for resilient democratic social relationships amongst the aims of landscape anthropology and archaeology.

Recently Latour (2008) has suggested that a number of highly promising orientations towards experimental practices are emerging, which have connotations similar to the words, "design or redesign." Several aspects of such connotations relate closely to Wescoat and Erickson's efforts to integrate strongly reflexive experimental archaeologies into projects to sustain and enhance plurality of 'lived cultural heritage.' These include such connotations as:

a) "a humility that seems absent from… the Promethean sense of what it means to act" (Latour 2008, 2) such as the sense that has recurrently been associated with what Toulmin (1990) calls the "myth of the clean slate",

b) an attentiveness to details, and to the indeterminate multi-dimensionality of enskillment and creativity (Ingold 2000), which would be eclipsed by the "Promethean, dream of action – "Go forward, break radically with the past and the consequences will take care of themselves!" (Latour 2008, 3),
c) emphasis on historically contingent meanings ('matters of concern') being as real as brick, rather than in terms of dichotomies of nature-culture, reason-tradition, and the global, total and real versus the local and illusory" (Ingold 2000),
d) awareness that no process begins from scratch – to design is always to redesign, to experiment is always to revisit pasts that differ to the present and indeterminate futures – redesigning and cautious experimentality may be an "antidote to colonialising, establishing, or breaking with the past" – "to the search for absolute certainty, absolute beginnings, and radical departures",
e) emphasis on ethical issues, as suggested by the expression "good or bad design" and the question of for and by whom experiments are pursued.

For Latour, such strongly reflexive orientations are most remarkable. They combine such hitherto supposedly mutually exclusive orientations as innovation and precaution, revisiting the past and discovery, the real and the indeterminate – as though we had to imagine a Prometheus stealing fire from heaven in a cautious way!" Interestingly, such orientations may be motivating the work of a number of contributors to this volume.

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6. Experiment or Demonstration? Making Fermentable Malt Sugars from the Grain and a Discussion of some of the Evidence for this Activity in the British Neolithic

Merryn Dineley

I have been investigating traditional and ancient techniques of brewing for several years. What began as experiments in 1995, when I used beeswax or fat-sealed pottery bowls, crushed malted barley and open fires to make a sweet mash in my back garden, has now become a demonstration (Dineley 2000, 2004). I have repeated the original experimental work many times. It is successful every time because there is a scientific reason why these simple techniques work. If anyone were to follow the fundamental principles then they would easily be able to replicate my work, provided that the necessary conditions are adhered to. These are that the crushed malted grain is heated to temperatures of between 65 and 67 degrees centigrade (Line 1980, Hough 1985). Thermometers are not necessary – the temperature should be just too hot for comfort. You can see, smell and taste the saccharification within minutes of beginning to heat the crushed malt.

Grain starch can be converted into malt sugars and ale using a variety of equipment and many different sources of heat (Hornsey 2003). The equipment used may differ but the biochemical processes within the grain remain the same. This is the aspect that I want to discuss in this paper.

Recently I took part in a 'Beer Forum' that was funded and organised by liveARCH (www.liveARCH.eu) and EXAR (European Association for the Advancement of Archaeology by Experiment). I travelled to the Eindhoven Open Air Museum, Holland, (http://www.historisch-openluchtmuseum-eindhoven.nl.) where, over two days, a small group of brewers, re-enactors, archaeologists and archaeobotanists met to exchange their experiences and knowledge of traditional, historic and prehistoric techniques of processing grain into malt, sugars, wort and ale.

Huib van der Stam demonstrated medieval mashing techniques, together with several brewing assistants (Figures 6.1, 6.2 and 6.5). I demonstrated prehistoric mashing techniques, working in collaboration with Brigitta Berszenyi and Zsuzsana Lehoczki, archaeobotanists from the Matrica Museum, Hungary's oldest open air archaeology

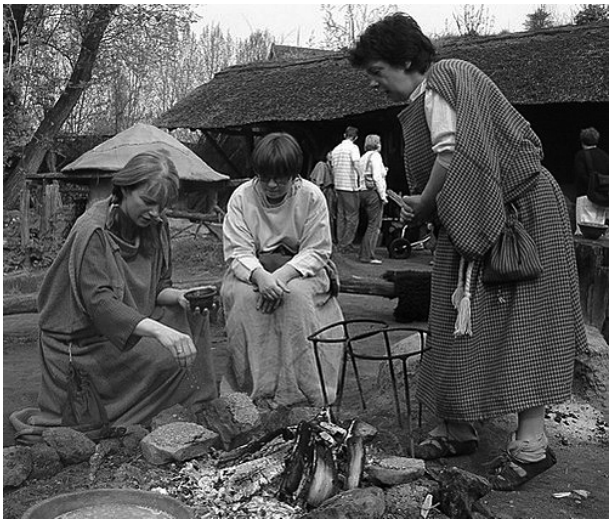

Figure 6.1. Huib van der Stam demonstrated medieval brewing techniques (top photo) and prehistoric mashing was demonstrated by myself (bottom photo, right, standing). Brigitta Berszenyi (seated left) and Zsuzsana Lehoczki (seated right) investigated making barley 'cakes'. Photos taken at Historisch Openlucht Museum Eindhoven, April 2009. Photos copyright Huib van der Stam and Merryn Dineley.

museum (www.matricamuzeum.hu). Brigitta and Zsuzsana were interested in making sweet 'barley cakes', a good source of B-vitamins and an interesting non-alcoholic use for a barley mash (Figure 6.1).

Huib van der Stam and a group of brewers demonstrate medieval mashing techniques several times a year at the Eindhoven Museum. They are successful every time because they understand the processes involved, they know exactly what they are doing and have done it many times before (Figure 6.2). These are demonstrations, not experiments, based upon knowledge, experience and skill. Knowledge comes from study and an understanding of the techniques and processes involved. Experience and skill come from practice.

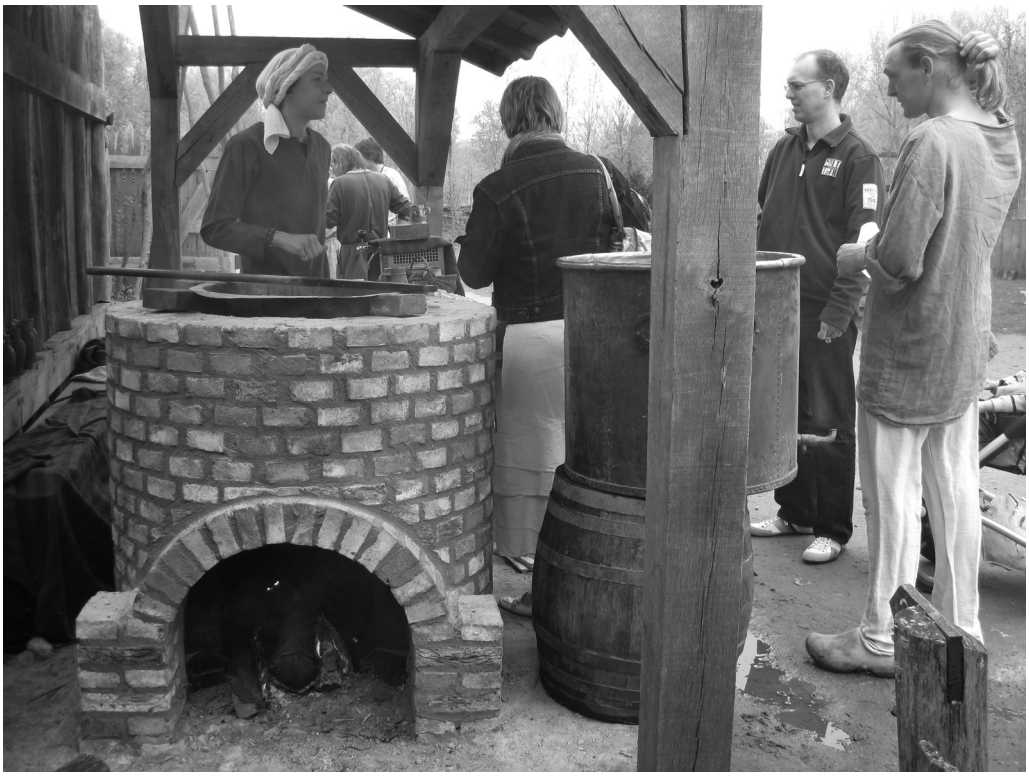

Figure 6.2. Mashing the medieval way: the brick structure houses the metal container in which the crushed malt and water are mashed. A fire heats the mixture. There is already some spillage of water, indicating the usefulness of drains in the brewing process. The mess increased as the day went on.

Four Norwegians from the Lofotr Viking Museum, Borg, Lofoten islands (http://www.lofotr.no/engelsk/) also attended the beer event. They had been making fermentable sugars from grain using the traditional technique of hot rock mashing. This involves dropping hot stones into a wooden vessel containing crushed malted barley and water. Obviously, barley mash can only be heated over an open fire if one uses pottery or metal vessels. If a wooden vessel is used then a different technique is required – hot rock mashing, a historic technique used until recent times in Central Europe (Vencl 1994). The participants from Norway used wooden tubs or half barrels and dropped heated stones into the crushed malt and water mixture.

Hot rock mashing is efficient. The technique was successfully demonstrated by Quinn and Moore at the Sixth World Archaeology Conference, Dublin, in June 2008 (see Quinn and Moore 2007). Their wooden trough replicated those found at Burnt Mounds or Fulacht Fiadh, as they are known in Ireland. These are large mounds of fire-cracked stones associated with a building and a large wooden or stone trough. They are usually close to a reliable water supply, such as a stream. They are usually dated to the Bronze Age.

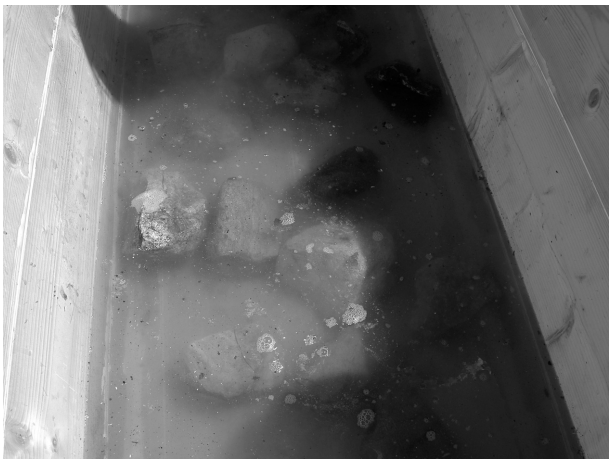

Figure 6.3. Hot stones are dropped into water in a rectangular wooden trough. This heats the water but does not make it boil, an ideal temperature for mashing. Photo: Merryn Dineley, Dublin WAC-6, 2008, Moore Group Demonstration.

Figure 6.4. Crushed malt barley is transformed into liquid malt sugars. This is the mash. The mixture is dark brown, caramelised and sweet. A hot stone has just been dropped into the mixture and the caramelisation of the mash can be seen, smelt and tasted. It really is a magical process. The sweet liquid can be fermented. Photo: Merryn Dineley, Dublin WAC-6, 2008, Moore Group Demonstration.

There are over 4000 of these sites in Ireland, several hundred in Orkney and they have been found throughout the British Isles and in Northern Europe. They have been interpreted as saunas, bathhouses or as places where large pieces of meat were cooked, even though the evidence for animal bone found in association with these sites is sparse. Other interpretations include felting or other wool processing activities, such as dyeing. The wooden or stone troughs found at these monuments are ideal for running a mash, as demonstrated by Quinn and Moore (Figures 6.3 and 6.4). They have repeated the technique many times with success (www.mooregroup.ie). Their work has been replicated by Gerard Flynn, who even malted the barley before mashing it successfully in a wooden trough using hot stones. He then fermented it in Bronze Age style pottery vessels (Flynn 2008).

Graham Dineley, my husband and a Craft Brewer of some 25 years' experience, and I have also successfully demonstrated this technique of hot rock mashing at the Visitor Centre for Tomb of the Eagles and Liddle Burnt Mound, South Ronaldsay, Orkney, in the summer of 2008 as part of an event to celebrate the 50th anniversary of the discovery of these sites by the local farmer, Ronnie Simison (Dineley 2008). We used a wooden trough with a volume of around 10 gallons, commensurate in volume to a large Grooved Ware pot, such as are found at many domestic and ritual Neolithic sites throughout the British Isles. We heated crushed malt and water with hot stones to make a mash. It began to ferment overnight as natural airborne yeasts began to transform the sugars into alcohol.

The event at Eindhoven was a rare opportunity for me to discuss brewing techniques with other like-minded people. We learned a great deal from each other. I have been aware for some time of the variety of brewing equipment that has been used over the millennia. The simple processes of mashing and fermentation can be done with different and varied equipment. This fact was demonstrated to good effect at this meeting and it might explain why archaeologists have such a problem in the identification of brewing as an activity at prehistoric and historic sites.

Other important aspects that we discussed were the importance of access to water and the usefulness of drains in this kind of wet processing grain activity. Transforming grain into liquid malt sugars and ale is a messy business (Figures 6.2 and 6.5). Vessels must be washed prior to use and after; there are always spills of the sweet, sticky wort as it is transferred from one vessel to another for fermentation – and all this before a single drop can be drunk!

My first experience of domestic, small scale brewing was over 25 years ago, watching home brewers convert crushed malt into sugars for fermentation. Simple ingredients and equipment were used – water, crushed malt, a mash bag, a plastic bucket for the mash and a heat source, usually electricity or gas, as well as yeast, hops and vessels for the fermenting wort (Line 1980). Medieval brewers did exactly the same thing, the

Figure 6.5. Making ale is a messy business. The medieval brewers at Eindhoven made more and more mess as the day wore on. Drains are useful. Photo: Merryn Dineley.

only difference being that they used fire as a heat source (Figure 6.2). The process of mashing or saccharification within the container was exactly the same.

The principle and essential ingredient for beer is malt – grain that has been slightly germinated, dried then lightly crushed. The traditional way of making malt is to steep the grain then spread it out upon a malting floor until the root is about one third of the length of the grain (Hough 1985). Germination has clearly begun and the malt is then kilned at low temperatures. This prevents the enzymes from being destroyed. When thoroughly dried, the grain can be stored for later use. It is best crushed just before being mashed. When crushed malt is heated the enzymes re-activate and continue the conversion of starch into sugars – this is the saccharification or mashing process. Temperatures of between 65 and 67 degrees centigrade are required.

The biochemistry of malting has only been understood within the last fifty years or so, when the role of the growth hormone gibberellin to activate enzyme activity within the grain was discovered (Hough 1985, 25–40). Crushed malt can be mashed using a variety of equipment but the basic principle remains the same: gently heat the crushed malt with water in a mash tun.

I used pottery bowls and deep bag-shaped pots over hot ashes and charcoal. The pots had been made by Flor Buchuk, the resident potter at the Eindhoven Museum, and they had been burnished to make them watertight as well as being fired at low temperatures so they would withstand the heat of the fire.

In my bowl and deep pot mashing demonstration the process of saccharification was, of course, taking place. The crushed malt mixture began to sweeten within minutes. The transformation from starch to malt sugars is seen in the black and white image – white starchy crushed malt is within pots beside the fire and the dark brown, delicious sweet mash is in the bowl on the fire (Figure 6.6). The longer I left the mash on the fire, the more caramelised it became (Figure 6.7).

Visitors tasted it and were amazed at how deliciously sweet it was. There is so much more to mashing than simply making wort that can be fermented into beer or ale.

Figure 6.6. Mashing in pottery vessels containing crushed malt and water. Hot ashes and charcoal provide the heat. Beside the fire are bowls of pale crushed malt. The mash in the bowl on the fire was beginning to caramelise – it had been gently heated for an hour and a half. The deeper pot was used and a sweet mash was made successfully. The shape of the container does not affect the mashing process. Photo: Merryn Dineley.

Figure 6.7. Bowl mash and barley cakes on hot stone: two different techniques for making sugars from crushed malt. The barley mash in the bowl is dark brown, sweet and caramelised – it is malt extract and it tasted delicious. The barley 'cakes' were also dark brown and sweet. Photo: Merryn Dineley.

The sweet mash and liquid are nutritious in their own right, rich in B Vitamins and delicious. Malt Extract is often used in breakfast cereals, confectionary and Ice Cream – have a look at some food labels for such products and you will see how commonly it is used in the food industry today.

Brigitta was interested in how the sweet 'barley cakes' were made. She found some flat stones and we made 'biscuits' or 'cakes' using a crushed malt and water mixture. They looked like granola or grain bars that can be purchased today. We kept them moist by sprinkling water on them and, sure enough, they quickly began to sweeten and caramelise as they warmed up – they became tasty, sweet and crunchy (Figure 6.7). It would be possible to add nuts and/or fruit or berries and Brigitta is going to experiment with these possibilities in the future to see what she can make.

This kind of grain processing – small scale mashing to produce sweet barley mash and a few gallons of wort, suitable for local or family consumption – has taken place in a domestic context for many thousands of years. Indeed, the processing of grain into sugars probably had its origins in the Fertile Crescent of the Ancient Near East c.10,000 years ago (Dineley 2004). The processes have been industrialised only recently (Hornsey 2003). Few people today ever have the opportunity to see these fundamental grain processing techniques and even fewer understand the principles behind them – the crafts of traditional floor malting and home brewing from the grain may become lost skills. I am fortunate to be married to a Craft Brewer who regularly transforms crushed malt into sweet wort and ale and so I am familiar with the necessary techniques. Practical demonstrations like those that I have described illuminate the archaeological evidence and allow for a better understanding of grain processing activity in history and prehistory.

The products of the brewing process – malt, sugars, wort, ale and spent grain – are ephemeral. They are consumed. The spent grain, the only potentially 'waste' product in this process, can be fed to domesticated animals or consumed by birds, worms and other wildlife (Dineley 2006). There is very little tangible evidence for the conversion

of grain into ale in the archaeological record. In order to be able to recognise what little there is one needs to be thoroughly familiar with the processes involved. There is no substitute for getting one's hands sticky and actually doing it.

Unprocessed or unmalted grain is not digestible. Cereals are malted to make them more palatable. This makes them attractive to many organisms – cattle, pigs, goats and horses (but, strangely, not sheep) love malt and spent grain. So do rodents, birds, earthworms and of course moulds. Carbonised grain does occasionally survive in the archaeological record of the British Isles and Northern Europe. There are several interesting examples of such finds that indicate that malting was a grain processing technique familiar to and regularly practised by people in the Neolithic.

The site of a huge timber building dated to c.4000 BC was excavated at Balbridie, in the Grampian region of Scotland (Fairweather and Ralston 1993). Thousands of carbonised grains were found and, several years ago, Professor Ian Ralston gave me six of these to investigate. It has taken some time to access a scanning electron microscope but, finally, I am able to publish such a picture (Figure 6.8). The embryo of this tiny 6000-year-old carbonised grain is completely and very neatly missing. This was not done mechanically, using Stone Age technology. The grain is germinated or malted. There are other examples of carbonised grains that are missing the embryo, for example, 32 fragments of charred grain with embryos missing were noted during excavations at the site of a rectangular timber building at Tankardstown, Co Limerick, Ireland, dated to the early Neolithic (Gowan 1988). The embryo is the first part of the grain to be hydrolysed by enzymes during germination. This evidence of grain with missing embryos indicates malting. Glumes and spikelets were also found in and around the building – processing waste that is produced when threshing and winnowing the harvested grain. This building served as a grain barn.

Figure 6.8. Scanning Electron Microscope image of a 6000-year-old carbonised grain from Balbridie, Grampian, Scotland. The embryo is missing. This is germinated or malted grain. Grain sample provided by Professor Ian Ralston. Image courtesy of the Satake Centre for Grain Process Engineering, University of Manchester, England (Formerly UMIST).

The building at Tankardstown, when standing, was around 7 metres long by 6 metres wide. A great deal of organic material was recovered during excavation including hazelnuts, dried wild apples and crab-apple seeds as well as carbonised grain. This would indicate the use of the building for the general preparation of food as well as a grain barn. Initial examination of the botanical samples describes the grain as having been "rendered down to a fragmented state before charring" (Monk *ibid*., 186). The floor was of beaten earth and suitable as a malting floor. The necessary equipment for drying the malt, making a sweet mash and fermenting the wort was available within this building. There was a hearth or oven, evidenced by an area of oxidised clay, and sherds from pottery bowls were found. The destruction of the building by fire is consistent with an accident, probably during the malt-kilning stage.

Occasionally there is mention in excavation reports of grain that has been 'dehusked', for example, at Barnhouse, Orkney, an early Neolithic village located half a mile away from the Stones of Stenness (Richards 2005). Barley husks were found in Structure 2, evidence of grain processing activity. Dehusking involves the removal of the hard grain husk from the starchy endosperm of the grain. Today this is done with huge steel grinders – unmalted grain is tough and it is impossible to remove the husk using simple stone tools, such as a pestle and mortar, pounder or saddle quern. Pounding unmalted grain with a pestle and mortar creates grits. However, when grain is malted, enzymes break down the husk naturally and the husk practically falls off the malted grain; the task of separating grain from husk is then trivial.

Malting is an efficient and easy way of dehusking the grain. All you need is a well-drained, smooth floor surface within a building. At Barnhouse Structure 6 had a smooth clay floor surface that had been repaired and extended several times. The floor was not level but slightly concave – good for drainage when watering the malt. The Neolithic Orcadian villages of Barnhouse and Skara Brae have well made floors and extensive drains as well as Grooved Ware pottery. Excavations at the Neolithic settlement at the Ness of Brodgar, close by the Ring of Brodgar, have revealed buildings and drains as well as many sherds from large Grooved Ware vessels and there is access to a reliable water supply – the Loch of Harray. The remains of a very large building was revealed in the summer season of excavation in 2008 (Card *et al*. 2007, www.orkneyjar.com). Structure 10 appears to have been about 20 metres in length – by far the largest stone building dated to the Neolithic that has been discovered to date. These buildings were not all dwellings but it is as yet unclear what their function was. Excavation of the Ness of Brodgar is not yet completed but it is thought to be more than an ordinary village.

The site of a Neolithic village dated to the 4th Millennium BC was excavated at Lough Gur, Knockadoon, Co Limerick, Ireland (O'Riordain 1954). The stone footings of circular and rectangular timber buildings were visible before excavation. Building 'A' was a substantial rectangular timber-walled building with a paved area, originally larger than that which survives. It would have been suitable for threshing and winnowing grain. Gaps in the wall footings, one in the southeast corner and the other half way along the west wall, would have provided the necessary through draft. There was a deliberately made hard clay floor area within the building, suitable as a malting floor. Sherds of round bottomed bowls and large flat bottomed pots were found as well as a

burnt area in the northwest corner of the building, which might signify the presence of a hearth or oven. Traditionally, barns are multi-functional buildings and Building A was no exception. It may have been used as a tool and equipment store or as a workshop and meeting place as well as functioning as a grain barn, malting floor and store for the Neolithic community. The building was destroyed by fire, a common fate for barns throughout history.

So, it is clearly a mistake to interpret all buildings that are excavated as 'houses'. This has happened at Durrington Walls, a large Neolithic henge monument located just a few miles from Stonehenge and yards from Woodhenge. Durrington Walls was originally excavated by Geoffrey Wainwright who found buildings, timber circles and sherds of Grooved Ware and Beaker pottery in abundance (1971). Recent excavations by the Stonehenge Riverside Project (2005–2008) have revealed 'houses'; the larger ones are interpreted as dwellings for important people. All have deliberately made plaster floors, some are described as concave, and they have all been interpreted as 'houses' or 'dwellings'. Such floor surfaces would have been ideal for use as malting floors and the presence of both Grooved Ware and Beaker pottery indicate brewing activity here. Food and drink preparation areas and suitable buildings would have been necessary if large scale feasting took place at Durrington Walls, as has been suggested by Wainwright, Parker-Pearson and Thomas, amongst others. It is unlikely that all buildings at Durrington Walls were 'houses'.

The excavators have indicated to me that they believe that there was no grain processing activity at Durrington Walls because they did not find any grain during their excavations (pers. comm. Julian Thomas, Mike Parker-Pearson). Parker Pearson has suggested the possibility of cider making on the basis of the discovery of a few crab-apple pips, although how cider could have been made with sour crab-apples and a lack of suitable crushing equipment has yet to be explained.

There is a very simple explanation for the lack of grain evidence at this important Neolithic site. Pigs ate the spent grain. The pig teeth at Durrington Walls have caries, as if they had been consistently fed something sweet (Albarella and Serjeantson 2002). It has been suggested that honey was fed to the piglets, which seem to have been slaughtered around the age of nine months (Time Team 2005). Surely honey was far too valuable a resource to have been used as pig feed. Also, very large amounts of honey would have been required for such a purpose. A much more logical explanation is that the piglets were fed spent grain, the residue and waste by product from making ale or beer (Dineley 2006). Spent grain contains residual sugars and is sweet and sugary. It has been used as animal feed for a very long time.

An important potential source of evidence that proves the processing of grain into malt, wort and ale can be found in the pottery vessels. At Barnhouse, Orkney, analysis of the Grooved Ware has revealed the presence of barley lipids and also unidentified sugars (Jones 2002). Barley lipids are only liberated from the grain husk during the latter stages of sparging the mash – this is the process of washing hot water through the mash to obtain a quantity of sweet liquid wort. The sugars derive from either milk processing or from barley processing, but they have not yet been clearly identified.

In my mashing demonstrations I have used pots that had been sealed in several ways. Some were fat-sealed with butter or lard. Others had been sealed with beeswax.

Both techniques were equally effective. The identification of animal fats in the fabric of pottery vessels does not necessarily indicate the use of that pot to cook particular meats (Evershed *et al*. 2001). It might simply mean that the pot was sealed and made waterproof with such fats and it may have been used for a variety of cooking or food processing activities. I have used fat sealed pots and beeswax sealed pots to make sugars from the grain and, obviously, there are many other possible uses. It would be interesting to smash pots that have been variously sealed, then used for a specific purpose and analyse them to see what chemical evidence might remain within the fabric of the vessel.

Ongoing research by Dana Millson at Durham University involves sealing small pottery vessels with fats and beeswax (Millson, forthcoming). The pots are then used in a number of different cooking and food processing activities. She has found that beeswax is not suitable for sealing cooking pots since the pot fabric spalls and flakes away when it is heated over a fire. However, they are ideal for a function such as fermentation. This is the direction that archaeological research should be heading – practical demonstrations and experimental work are important. They can illuminate and clarify many activities that took place in the past. For too long the findings of experimental archaeologists have been ignored or disregarded by some archaeologists in the academic community. The appearance of an Experimental Archaeology Session at the Theoretical Archaeology Group Conference, Southampton, (TAG 30) is encouraging. Hopefully, there will be more collaboration, discussion and interaction between archaeologists who have different approaches to researching the past. Other disciplines, such as biochemistry, agricultural science or brewing technologies can play their part in formulating an understanding of the past. A multi disciplinary approach is both necessary and productive.

This paper has investigated and discussed some of the practical aspects of brewing in history and prehistory and it provides some markers and indicators that provide evidence for malting, mashing and brewing. Malting floors have changed little over the millennia and traditional techniques still require a smooth, level floor surface that is slightly sloped for drainage and is protected by a building. Modern techniques of Saladin Drums and Germinating Kilning Vessels were introduced to the malting industry only fifty or sixty years ago. Dehusked grain is an indication of malting. Charred grain with missing embryos is also an important marker of malting, since it is impossible to remove the embryo from a tiny grain mechanically. Vessels for mashing and fermentation can be made of metal, wood or pottery. The one invariant involves the changes within the grain itself during processing and the ensuing chemical evidence that brewing creates. Barley lipids and sugars within the fabric of pottery vessels are a significant indicator of brewing.

Feasting in the Neolithic has been recognised as an important social and ritual activity. The manufacture of ale that was consumed at these feasts is also an important aspect of domestic and ritual Neolithic life. I hope that more archaeologists will investigate this element in the future. I am sure there is much more to be revealed.

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7. Experiments in Beaker Construction Techniques

Harriet Hammersmith

Abstract

Pots were made by people within certain parameters to perform a certain function or functions. An archaeologist studies an artifact, such as a pot, not to learn just about an artifact, but to try and gain understanding about the people who made and used that artifact. Studying the construction technologies of any artifact may lead to insights on how people solved the problems inherent in making an object. Much experimental research has been done along these lines, such as archaeologists who study prehistoric stone tools learning to knap and make the tools themselves. Research and experimentation has been done as well on prehistoric pottery technologies around the world and in Britain. By utilizing research on prehistoric pottery technologies and examining the physical evidence detectable on actual Beakers relating to those technologies, a theory is developed on a possible construction method. By examining Beakers themselves, indications concerning their construction may be detected. This visual evidence is then combined with a working knowledge of pottery and research into prehistoric pottery technologies. There are most certainly a number of viable techniques for making Beaker-type vessels; this research however focuses on the possibilities suggested by the visible indications of construction found upon examining actual Beakers. Experimental Beaker-type vessels were made utilizing this theory of construction testing the viability of the theory. This research proposes one viable construction method for British Beakers.

Introduction

Elegantly formed and intricately decorated, Beakers have fascinated archaeologists and been the focus of study for many years. Their seemingly sudden and widespread appearance in the archaeological record of Britain led to archaeologists forming theories of the "Beaker People." The fine workmanship of the Beakers led some archaeologists to postulate ideas that exceptional expertise or unusual techniques must have been employed in Beaker construction; and the decorative motifs on Beakers generated studies of typologies which were sometimes overly complex in their analysis. Not

all Beakers are of an exceptionally high quality, and Beakers are not the only well-constructed vessels in Britain. No special techniques or expertise are needed, however; for with care, well made Beakers were capable of being made with the potting techniques known to the prehistoric potters of the time (Gibson 2002).

Different theories have been put forth about who the "Beaker People" were. The Beaker People as invaders of Britain attacking and then dominating the indigenous population was a popular and long held view (Clarke 1970). Migrating peoples from the continent settling in Britain alongside the indigenous population is a more recent theory. These settlers would have brought their ideas and way of life and, over time, become integrated with the ideas and way of life of the indigenous population. The Beakers may simply have been the latest fashion in pottery brought by settlers and adopted by the local people (Case 1977); or it has also been suggested that Beakers were part of a new cult which merged with the religious practices already in place (Burgess and Shennan 1976). A Beaker may have been made by a settler or the descendant of a settler; another Beaker may have been made by an indigenous potter who saw a Beaker and copied the style using the techniques already known to him. Another possibility is that two potters, one indigenous and one a settler, may have exchanged knowledge and techniques with each other, each developing their own style of Beaker within the parameters of their community. Many questions remain unanswered about who the people were who made and used Beakers and why they made them. Much of the current Beaker research is focusing on European origins and migrations and new dating programmes which should shed more light on Beaker chronology in Britain. Beakers are known to have been used in burial practices and were most likely also used in domestic contexts as well (Case 1995; Gibson 2002). Whatever the reason, people in Britain made Beakers. This research is an attempt to discover how.

This research was undertaken as part of my dissertation for an MSc at the University of Edinburgh, and I originally trained as a potter for my BA from Columbus College of Art and Design. Being a potter myself gives me an understanding and insight into the thought processes relating to vessel construction and problem solving that any potter might confront, even pre-historic potters as the basic methods have changed very little. Having researched prehistoric pottery technology and learned what physical evidence is detectable on actual pots relating to that technology, I developed a theory on how Beakers may have been made and tested this theory by making my own beakers. There are most certainly a number of viable techniques for making Beaker-type vessels; this research however focuses on the possibilities suggested by the visible indications of construction found on Beakers I examined held by the National Museum of Scotland. I then proceeded to make some experimental Beakers utilizing my theory of construction as well as making a few Beakers with another method suggested by S. E. Van der Leeuw (1976).

Studying the construction technologies of any artefact may lead to insight on how people solved the problems inherent in making an object. Much research has been done along these lines, such as archaeologists who study prehistoric stone tools learning to knap and make the tools themselves. Research and experimentation has been done as well on prehistoric pottery technologies around the world and here in Britain. In 1976, S. E. Van der Leeuw published an article putting forth his ideas on the construction

technology of Dutch Beakers. Regardless of whether Van der Leeuw's theories are viable or not, British Beaker construction techniques may have been different than those on the continent as the construction methods may have been influenced by indigenous British potters. This research proposes one viable construction method for British Beakers.

Research

Beaker potters were indeed knowledgeable in preparing the fabric of their vessels, the formation of their vessels, and the firing of their vessels. I have examined ten beakers and numerous sherds in the collection of the National Museum of Scotland and, as a potter myself, was suitably impressed by the talent of these potters as evidenced by their handiwork. The Beakers, mostly of All Over Comb and All Over Cord decoration, varied in height from less than 10 cm to 20 cm. Profiles in shape varied from the classic Beaker "s" shape to Beakers with necks sharply angling out from the body. Some fabrics were thin and fine, while others were coarser. But regardless of decoration, shape, or fabric, all showed evidence of a degree of competence and knowledge on the part of the potter.

In studying the beakers, I paid close attention to any evidence of the formation techniques employed by Beaker potters. As the walls of the Beakers examined tended to be quite thin, on average about 5 mm, ring building alone probably would have been an inadequate method, as the walls would have needed to be thicker to support their own weight during construction and the pressure applied when adding the next ring. When examining the Beakers for evidence of formation techniques, I did look for attributes associated with ring building. Ring built vessels invariably have variations in wall thickness, sometimes nearly twice as thick at some points than in others (Rye 1981). Walls may also have a slightly corrugated feel unless ring junctions were carefully obliterated by smoothing or scraping. Another method for building Beakers, and incorporating the ring building technique, was proposed by Van der Leeuw (1976). He suggested the walls may have been supported by wrapping cordage or strips of a flexible material such as reed or leather around the vessel during construction. This method begins with a pinch pot and proceeds with the building up of vessel walls by ring building with rolls of clay. Evidence of a pinched vessel may show regular shallow depressions resulting from the fingertips pressing against the clay (Rye 1981). Rings of clay would then be added to make the vessel taller, the potter pushing the clay against the wrap of cordage to thin the walls. Van der Leeuw (1976) suggested that evidence for this method included the cord impressions themselves, wide gullies on the outside surface suggesting where a supporting flexible strip of material of some kind had been placed, and the banded decoration on some vessels. With these banded decorations, some of them had deeper impressions than other bands suggesting that the lighter impressions were made when the vessel was in a drier state, possibly after the wrap had been removed. Evidence of scraping and burnishing were also looked for in the Beakers examined. My own theory, prior to the examination of the Beakers, consisted of a vessel being ring-built then scraped internally upon reaching the leather hard stage when the vessel walls would have been stiff enough to support themselves when thinned. External scraping at this stage would result in surface gouges, caused

by bits of temper scraping across the surface, which would have been hard to eliminate even with wiping and burnishing. Van der Leeuw had dismissed internal scraping as a viable option because he could not fit his hand into some of the narrower Beakers. I was able, however, to adequately put my hand into the smallest Beaker, in which the internal neck diameter was approximately 7 cm. A child's hand would be smaller yet, and a child could be taught to do such a task as scraping. My theory changed however upon examining the Beakers.

The Beaker potters were talented and knowledgeable indeed. Even the coarser and less refined Beakers show a certain level of competence in fabric, formation, and firing. The property of thickness was certainly successfully manipulated by the potters. The Beakers examined had an average wall thickness of about 5 mm; some sherds were barely 3 mm. The tallest Beaker examined was 20 cm with walls approximately 8 mm thick. Wall thickness reflects not only building techniques but also the quality of the fabric. The fabric ranged from finely tempered to heavily tempered clays which could have an effect on fired strength. Although a fabric with more temper can make the clay stronger while building the vessel, a lot of temper can result in a weaker vessel after firing. The vessels were all adequately fired in an open fire, and went through the ceramic change as evidenced by the black core seen in most vessels. There were vessels with cracks, some extending from the rim possibly dunting cracks, which may have been the result of thermal stress during firing or while cooling. Some cracks and breakage may simply be the result of burial over time and not an indicator of thermal behaviour. Smoothing or burnishing of the surface may have been an attempt at reducing the porosity of the vessel, and if indeed there is a coating on some of the vessels then that may be evidence of a direct manipulation of porosity.

Prior to examining the Beakers, my theory of their formation process was that they had been simply made as a ring built vessel, then internally scraped thin when leather hard. After closely examining 10 beakers ranging from Clarke's early AOC to the later N3 category, I altered my theory. Most beakers showed evidence of beginning with a pinch pot, which became the bowl of the vessel from base to shoulder. This was evidenced in finger marks internally at the shoulder, difference in texture of the walls below and above the shoulder, difference in wall thickness below and above shoulder, and cracks or seams at the shoulder juncture both internally and externally. This evidence also suggests the pinched bowl may have been allowed to dry to the leather hard stage before the addition of more clay. Some similar variations in the internal wall surfaces between the shoulder and the neck and then the neck and the rim lead me to speculate whether the clay added on, from the shoulder to the neck, was again allowed to reach leather hard stage before the addition of the final rim section. This building in stages would make it possible for the potter to build a relatively tall, thinned-walled vessel with soft, plastic clay without risk of warping or collapse.

Experimentation

In order to test both my theory of building beakers in stages and the feasibility of using cordage as a support for the clay, I made nine beakers of various sizes and shapes. It was not my intention to accurately reproduce any one particular Beaker; however, I did

use the Beakers I examined as rough guides for the ones I made. I had closely examined ten Scottish Beakers held by the National Museum of Scotland. The Beakers examined were chosen as they were nearly complete vessels and they represented a range of examples from the different typologies. A list of these Beakers appears at the end of this article. My thought for the experimentation was that a prehistoric potter, familiar with making clay pots, may have seen a new style of pot which had perhaps come in through trade or as a gift or had been brought by new settlers, different from the vessels he or she was making. He or she may then have used their knowledge to attempt to make a vessel in this new style. As I was focusing on construction techniques, I used a manufactured crank clay finely tempered with grog which can be fired quickly at a low temperature attainable in an open fire. Utilizing my theory of the construction method evidenced in the Beakers examined, I attempted to achieve a similar thickness, while assuring the strength of the walls to achieve heights similar to the real Beakers.

Vessel One

The first beaker I made (Figure 7.1) was approximately 15.5 cm in height. A short, wide pinched bowl was made measuring just under 4 cm tall. After starting the pinch pot in my hand, I pressed it down onto a piece of scrap leather laying on a flat surface. This leather would allow me to easily turn the vessel while I worked. The bowl was decorated with comb impressions before being allowed to dry to the leather hard stage. After reaching the leather hard stage, the next section was then added using a strap of clay. The clay had been pressed out flat with the heel of my hand then a sharp wooden stick was used to cut out straps of clay about 4.5 cm in width which were then attached to the leather hard pinched bowl. To attach the wet, plastic clay straps to the stiff, almost dry leather hard clay, a sharp point was used to scratch lines in a criss-cross pattern on the rim of the leather hard clay and again on the edge of the soft clay strap. A wet slurry of clay and water was then rubbed onto both scratched edges. The edges were then pressed and squeezed together. The scratches not only provided a rough surface for bonding but also allowed some of the moisture from the slurry to be absorbed by the leather hard clay ensuring a stronger bond. The clay slurry also helped to fill in any small voids that could have lead to cracks developing during drying or firing. The addition of the strap took the height to the intended shoulder of the vessel. The comb impressions of the second banding of decoration were then added. The vessel was then set aside again to allow the new section to reach leather hard stage. So that the original pinched bowl did not become too dry risking cracking while the second section was drying, it was loosely wrapped in a slightly damp cloth. The third strap was added in the same manner building the beaker from shoulder to the narrowest part of the neck. After this section became leather hard, the fourth strap was added finishing off the beaker, and the final banding of comb impressions were done. I made no attempts to remove the seams between straps by scraping or wiping on the internal surface of the vessel. Externally, the seam between the pinched bowl and the first strap was deliberately left visible as part of the experiment, the next seam was scraped then wiped with a damp cloth to smooth out the scratches from scraping. The last seam was smoothed by scraping then wiped with a wet finger and lightly burnished with a

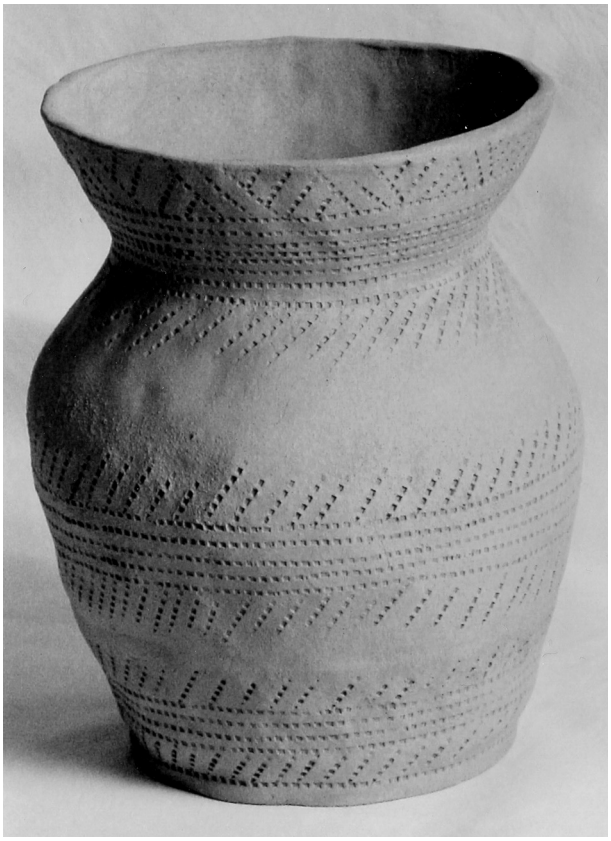

Figure 7.1. Vessel One – Section built, all over comb; 15.5 cm.

smooth wooden stick when leather hard. The beaker, with walls from 5 mm to 6 mm thick, is well able to be handled after fully drying and shows no visible cracking.

Vessel Two

The second beaker (Figure 7.2) attempted followed a very similar construction method in that it was again started with a pinched bowl and then three straps added in sections. The second strap again took the vessel to shoulder height before turning the walls slightly inward. This vessel, again approximately 15.5 cm in height with 5 mm thick walls, is wider with a slightly less pronounced angle on the neck than the last beaker. The shape was loosely based on a Beaker from Bathgate, West Lothian (Figure 7.3). The decoration was again done with a comb which I carved from a flat piece of wood. The comb impressions on the pinched bowl were made while the clay was still soft just after shaping the bowl; the rest of the impressions were made after the entire vessel was completed and had reached leather hard stage. Some pressure was necessary to adequately make the impressions and I noticed that while making vertical or diagonal impressions on convex areas of the wall that the pressure very slightly diminished the

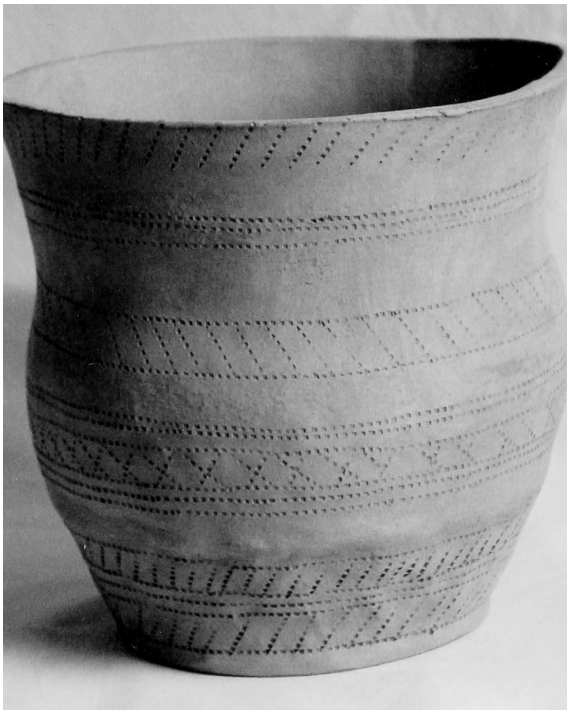

Figure 7.2. Vessel Two – Section built, all over comb; 15.5 cm.

Figure 7.3. Beaker from Bathgate, West Lothian; 19 cm.

outwardly curving silhouette. This slight alteration in the surface may account for the areas where Van der Leeuw would suggest a supporting strap of reed or leather may have been placed. As in the first beaker, some of the internal seams were left visible; the lower most seam was scraped at the time of formation, the upper two were left visible. Externally, the opposite was done, leaving the lower most seam visible, the upper two were scraped, then wiped.

Vessel Three

Experimental beaker number three (Figure 7.4) would be my first attempt with an all-over cord impressed beaker. The shape was based on the classic beaker 'S' shape. I made the 11 cm beaker in three sections, the first section being the pinched bowl to the height of the shoulder. The second section was a strap which reached about halfway between the shoulder and the rim. The third section was another strap which finished the vessel. All sections were allowed to dry to leather hard stage before the addition of the next section. The walls were approximately 5 mm thick. The cord impressions on each section were done while the clay was still wet just after the section had been formed. I made the cordage from cotton fibre of two strands twisted with a "z" or left spin (Hurley 1979). Starting at the base, I wrapped the cordage to the right. I held the cord to be wrapped in my right hand while pressing it against the clay with my left thumb. The fingers of my left hand were inside the vessel and while pressing externally with my thumb and internally with my finger, I was also able to turn the pot in a clockwise direction while it was sitting on a scrap of leather on top of a flat surface. The leather turned very smoothly; and as I was making the impressions while building the pot in sections, I had no trouble in reaching my fingers and thumb to the lowest part of each section. The length of the cord was approximately 65 cm long and coiled around the vessel about 4 times. When I ran out of cord, I unwrapped it from the vessel and, starting again where the impression stopped, I continued wrapping. A Beaker from Aberdeenshire on display in the National Museum of Scotland had a gap in the lines of cord impressions at the shoulder (Figure 7.5), so I spaced my impressions in a similar manner. This also made it easy to attach the next strap of clay without the worry of smudging the impressions. The impressions near the next seem did get slightly smudged when adding the last strap of clay. I would attempt to address this problem in another beaker.

Vessel Four

The next beaker I made was an attempt to make a taller beaker by starting with a taller pinched bowl. In the Beakers examined, on a number of occasions I noted finger marks and variations in wall thickness at the shoulder or widest part of the body of the vessel suggesting this was the rim of a pinched bowl to which more clay was attached. A pinch pot is restricted in height by the length of the potter's fingers, for the potter must be able to reach the internal and external base with fingers on one side of the wall and thumbs on the other to form the vessel (Van der Leeuw 1976). I made the pinched bowl for this beaker approximately 7 cm tall which is about the highest

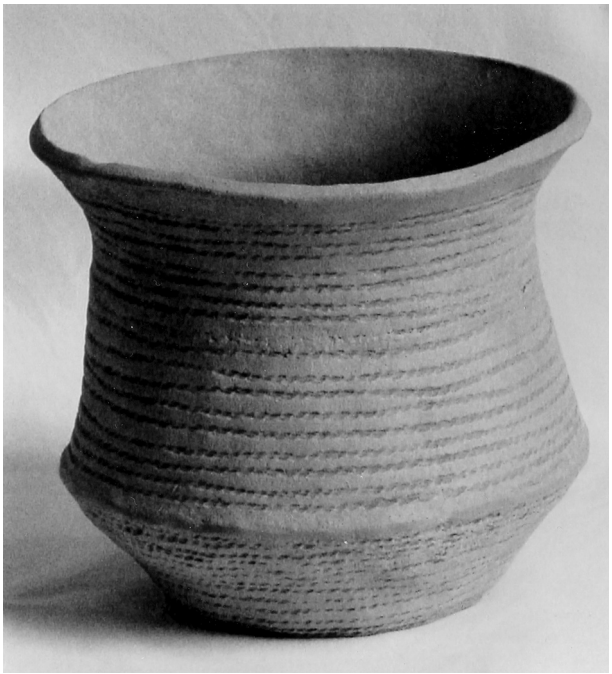

Figure 7.4. Vessel Three – Section built, all over cord; 11 cm.

Figure 7.5. Beaker from Aberdeenshire; 12 cm.

pinch pot I can achieve. I intended the rim of the pinched bowl to be the shoulder height, but as I was concentrating more on achieving height in this beaker, the shape became a little distorted from the shape I had in mind. Three more straps were added to the bowl in the same manner as the previous beakers, allowing for drying to leather hard stage before the addition of the next section; the overall height for the vessel is approximately 19 cm. The comb impressions were done in sections while the clay was still quite plastic. The seams, both internally and externally were scraped then wiped at the time of formation. Building in sections, waiting for the previous section to reach the leather hard stage, made for very strong sturdy walls. The wall thickness of this vessel varies between 4 mm and 5mm thick. The walls would have easily supported the addition of more straps.

Vessel Five

Wanting to achieve a similar height but with a more pleasing shape, the next beaker I made was based on the shape of a Beaker from Kelso (Figures 7.6, 7.7). This Beaker is slightly narrower for its height than many other beakers. Its rim is missing and the Beaker measures approximately 17.5 cm from the base to the highest point of breakage. It may have originally reached 20 cm or more. The beaker I made does reach 20 cm and began with a pinched base of approximately 7 cm tall. The walls again average between 4 mm to 6 mm in thickness, and were built in sections with straps of clay. External seams were scraped then wiped during construction; internal seams were left visible. The decoration, a combination of comb, fingernail, and spatula impressions were done in sections while the clay was still fairly plastic. Again the walls were strong and sturdy and would have easily supported additional clay when in the leather hard stage of drying.

Vessel Six

The next experimental beaker I made was my first attempt to create a vessel with cordage used to support the clay during construction, after the method described by Van der Leeuw (1976) (Figure 7.8). I began by making a pinch pot in my hand. When that became an adequate size I pressed it down onto a scrap of leather. I then placed the end of a cord just under the base and began to wrap it around the bowl with my right hand while pressing it in and turning the vessel with my left. I had about 20 to 30 cords approximately 50 cm in length prepared for this exercise. When I got to the end of one cord, I took another and, starting where the last one left off, I continued wrapping and pressing. I stopped wrapping about 1 cm below the rim of the pinch pot. Then placing my thumbs inside against the wall farthest from me and my fingers on the outside of this wall against the cords, I began to press and pull the clay upwards, thinning it out, and pulling more clay to the rim. I wrapped a couple of more cords around and pressed and squeezed again until the original pinch pot rim could be thinned out no more and was now about 8.5 cm tall. I then made a roll of clay which I added to the soft clay vessel by pinching and smoothing the seam. More cord was added to support the clay walls while I pinched and pulled to thin the

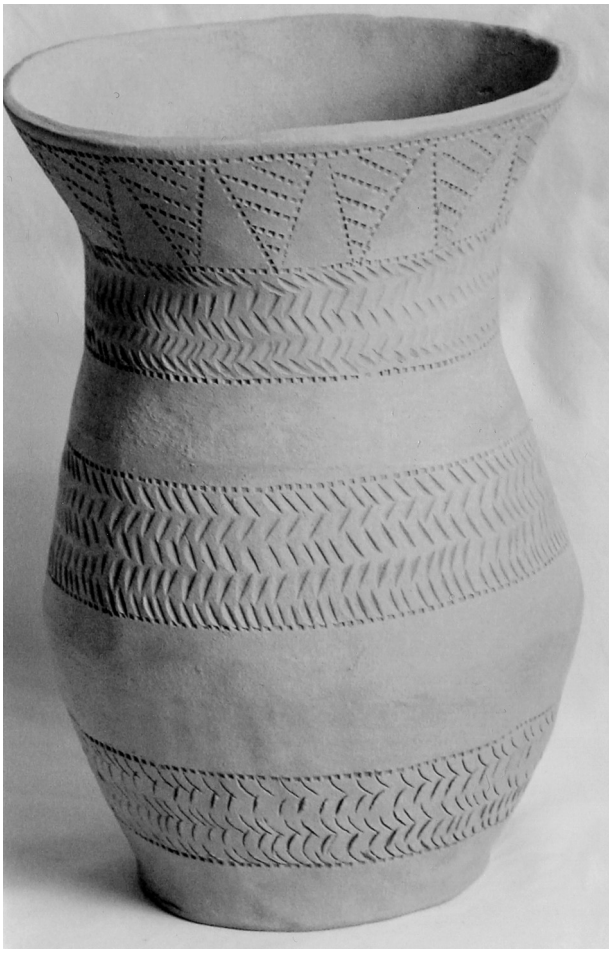

Figure 7.6. Vessel Five – Section built, comb and spatula; 20 cm.

walls out. I found that while this was a fairly decent way to make thin walls with soft thick rolls of clay, it was difficult to form an inward slope to the walls if I was exerting outward pressure to thin them. The walls were 4 mm to 5 mm thick. The cord ends also tended to pull apart, and the lines of cord impressions had a wavy look where clay had pressed out from between. When finished, I set the vessel aside to dry with the cords still wrapped around it. With the cords on, it took longer to dry than the other vessels; however I did take the cord off before the clay had fully reached the leather hard stage. This beaker was only 13 cm tall and the clay, while still a little plastic, was stiff enough not to need the cord for support by this time. I also wanted to test how easy it would be to smudge the clay enough to eradicate a deep cord impression. This was fairly easy to do. I did notice some areas where the surface of the clay had some fine tiny cracks which formed when pressed against the cord. The surface of the clay pinch pot had become slightly dry while was I preparing the next roll of clay to be

Figure 7.7. Beaker from Littleton Castle, Kelso; 17.5 cm to break.

added; the cotton cordage may have absorbed some of the moisture from the clay, as well, contributing to the surface dryness. A few larger cracks also developed during drying along the seams of the rolls of clay added to the pinch pot. This was my first attempt at this method of construction and the pot itself is rather disappointing. Two more attempts using this method would be made.

Vessels Seven and Eight

With the next two beakers using the wrapping method, I concentrated much more on achieving a more pleasing sinuous "s" shape (Figure 7.9). Although I was more successful in achieving the desired beaker shape, it was still very difficult to get an inward angle to the walls while thinning out the clay by pressing against the cords. In reality, the walls sloping inwards were formed independently of the cords, the cords providing very little support. The walls are also slightly thicker where they slope in compared to the walls from the base to the shoulder and the rim; overall wall thickness varied from 3 mm to 5 mm in both vessels. These are small vessels measuring only 11 cm and 10 cm. More care was taken when adding rolls of clay to ensure proper bonding. No cracks developed in the seams during drying. The external walls were burnished

Figure 7.8. Vessel Six – Cord supported ring built; 13 cm.

with a smooth wooden stick while leather hard, slightly flattening the ridges of clay between the cord impressions but not obliterating the impressions themselves.

The cord impressions on AOC Beakers are very regularly spaced and very horizontal, made by coiling the cord around the vessel. In making the experimental beakers, pressing the clay against the cords caused the clay to squeeze out between the cords, distorting the evenness of the impressions. Although the cords did offer an adequate support for the clay, the coiled cords shifted slightly and created variations in the spacing and wavering the impressions. When using this cord supported method of construction however, I felt more as if I was forcing the technique to fit the theory. As a potter, this method seemed an awkward way to work.

Vessel Nine

The final beaker I made was an all-over cord impressed beaker built in sections, continuing with what I had learned from the earlier experimental beakers (Figure 7.10). The original pinched bowl reaches the shoulder of the vessel at the widest point

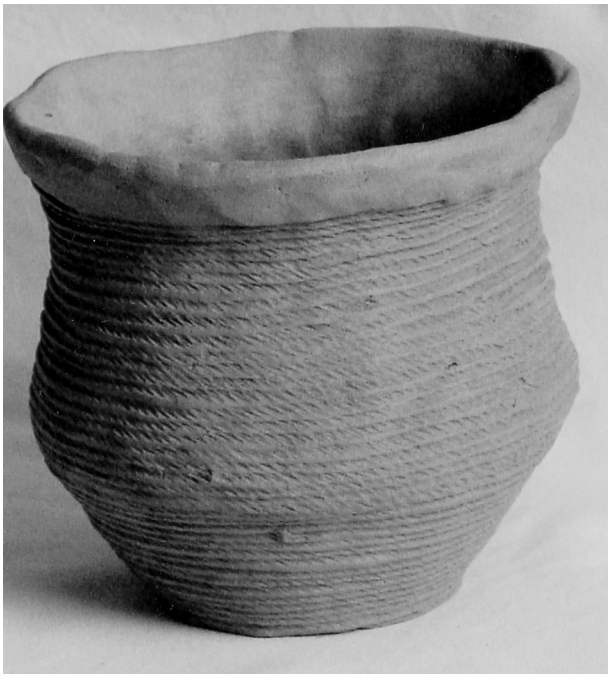

Figure 7.9. Vessel Eight – Cord supported ring built; 10 cm.

of its body. The cord impressions were made almost to the rim of the bowl while the clay was still plastic. This bowl was then set aside and allowed to dry to the leather hard stage. The edge of the rim was then scratched, clay slurry added, and a strap of clay attached. Care was taken when adding the strap to smooth out the seam and not smudge the cord impressions near the rim of the bowl. This took the vessel to a height just below the narrowest part of the neck. Taking advantage of the soft clay along the seam, the cord impression was continued where it had been left off on the pinched bowl to near the edge of the new rim of the second section. The vessel was set aside again with the original pinched bowl lightly wrapped in a damp cloth to prevent its over-drying. When the second section was leather hard the next strap was added in the same manner taking the height to its rim, again with care not to smudge the cord impressions near the new seam. The cord impressions were continued up over the new seam to about 1 cm below the rim. The seams inside were not scraped but were wiped with a piece of damp cloth. The walls are of a fairly even thickness of about 4 mm to 5 mm, and the height is just under 13 cm tall. The external walls were lightly burnished when leather hard. The cord impressions on the beakers in which the cord was used as support were visibly deeper as a result of pressing the clay against the cords, than the impressions made on the beakers on which they were mere decorations. The cotton fibres of the cordage were fairly soft; and cordage made out of stiffer plant fibres could leave a deeper impression when pressed onto clay.

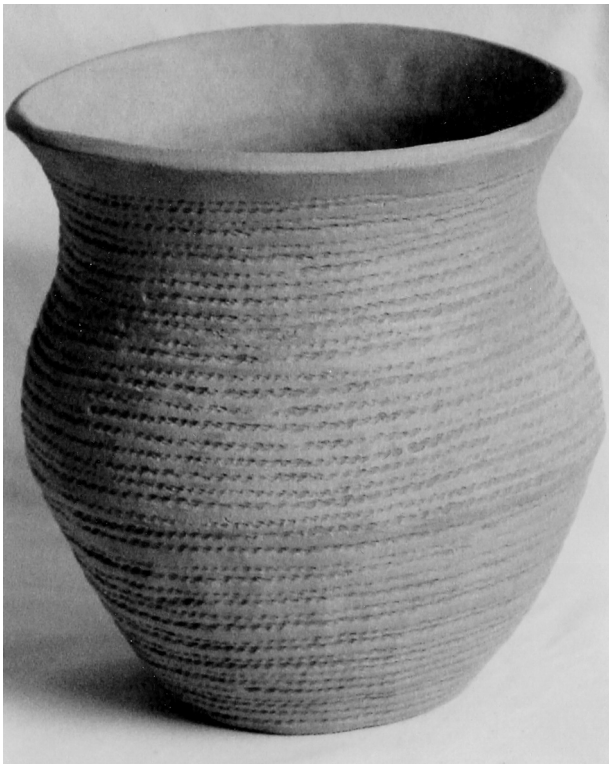

Figure 7.10. Vessel Nine – Section built, all over cord; 13 cm.

Results

Through this experimental work, I was able to create beakers with a similar thickness of wall to the Beakers examined of between 4mm and 6mm. The walls were more than sufficiently strong to support their height and even while in a fragile, dry unfired state endured being moved and handled quite a bit during examination and storage. Using a manufactured clay fabric, the porosity was fixed and the permeability was only addressed in the light burnishing some vessels received. All beakers were given a slightly concave base by thumping on the base with the heal of the hand when the vessel was stiff enough to be gently handled but just prior to the leather hard stage. This helps reduce the potential of the base cracking from horizontal shrinkage of the clay. When the clay was stiff enough, the pots were turned over onto their rims to promote the drying of the base. The vessels came through the crucial drying process successfully with the only cracking occurring on a seam of added clay on the first experimental cord-supported beaker. The experimental beakers were then pit-fired at Archaeolink in Aberdeenshire. Pit-firing often results in some vessels cracking and breaking which can occur for a number of reasons such as too-rapid heating or cooling, shifting of fuel and/or pots during firing, or, of greatest interest, weak joining along seams of clay. As most of the seams on the experimental beakers were left visible either

internally or externally, cracks along seams would be easy to identify after the firing. Upon examination I was pleased to find that most of the seams of the added straps came through the firings intact as did the cord-supported beakers. Building beakers in stages would seem to be a viable construction method.

The next step in expanding this research would be to focus on similar types of beakers from a single area, coordinating this with new radiocarbon dates becoming available. It may be possible to detect differences in construction techniques in different areas in relation to different time periods. More extensive research over wider areas across the entire time period encompassing Beakers would then need to be done. In this way a broader picture should begin to take shape of the way the people who made the Beakers were either, directly or indirectly, affecting the properties of ceramics through fabric, formation and firing. By combining research on Beaker pottery technologies with carbon dating being done by the National Museum of Scotland and the Britain-wide Beaker Isotope Project and any research which may be done toward finding origins of clay used in Beakers, the fullest picture yet possible will begin to emerge on the people behind the pots.

Pots were made by real people and it is ultimately the people about whom we as archaeologists are seeking knowledge; and this knowledge is sought by studying the evidence and artefacts those people have left behind. Pots do not equal people; they are not objects that are the fossilized culture or identity of a people (Boast 2002). Pots were made by people within certain parameters to perform a certain function or functions. An archaeologist studies an artefact, such as a pot, not to learn just about that artefact, but to try and gain understanding about the people who made and used that artefact. Why did they make it? How did they make it? How did they use it? These are just a few of the questions asked of an artefact. Beakers have fascinated people for many years because of their elegant shapes, fine craftsmanship, and often complex decorations. However, not all Beakers are finely crafted; some are thick-walled, lopsided and haphazardly decorated. Beakers are not the only British pottery to be so well crafted either; some examples of Grooved Ware and Peterborough bowls, as well as others, show considerable skill on the part of the potter (Gibson 2002). For all the studies done on Beakers over the years, from Abercromby to today, there is still so much that is not known about the people who made and used the Beakers. The Beakers may have been brought to British shores by settlers or arrived first through trade. The indigenous people of this island may have copied and adopted a new style of pottery as part of a new fashion or cult. Maybe the Beakers were brought by invaders. It is not even certain what the Beakers were used for. Even radiocarbon dating, such as the programme undertaken in the 1980's (Kinnes *et al.* 1991), has failed to establish a firm chronology for the various styles of Beakers found in Britain. It is to be hoped that the Beaker dating programmes being undertaken by the Britain-wide Beaker Isotope Project and the National Museum of Scotland will not only give a more firm chronology, but also shed light on the lifestyle of the people themselves by use of the isotopic analyses to possibly determine whether the person buried with a Beaker was born in the same area.

Conclusion

Studying how people made these pots is an attempt at trying to understand their problem solving abilities. The potter wants to make a pot with very thin walls of a certain shape but with enough strength to support itself during construction. How would the potter address this problem? Certainly there is more than one way to construct a Beaker-type vessel using the technologies available to the prehistoric potter. By examining Beakers themselves, indications concerning their construction may be detected. By combining this visual evidence with a working knowledge of pottery and research into prehistoric pottery technologies, I was able to successfully fashion six beaker-type vessels by building in sections beginning with a pinched bowl. This technique also had an unforeseen benefit of time management. Making vessels by building with rolls of clay in the ring building method necessitates the potter work on the pot from beginning to end. The clay of the vessel, if left even for a short time, can dry slightly; and this drying can cause the surface of the clay to become cracked and rough when the next roll of clay is added. It can also cause poor bonding of the two surfaces. So the potter has to be able to finish the pot he has begun. Building the vessel in sections, however, meant that I could spend a short amount of time working on a section, set it aside, and then work on it again later at my own convenience. As long as I made certain the vessels under construction were adequately wrapped to prevent too much drying, the vessels could wait. One pinched bowl sat for five days before I was able to work on it again; and I usually had three or four vessels under various stages of construction. The necessity of the prior section being leather hard before the addition of a new section makes for a convenient stopping point. Wrapping a ring built vessel with a damp cloth, even if then wrapped in leather, would not keep the clay adequately soft and moist for very long. Being able to set a pot under construction aside would be a benefit to a potter who may also have other demands on his or her time such as tending livestock or preparing food.

With skill and care any number of techniques may have been utilized by prehistoric potters to create Beakers. Building in sections is one possible method that adequately addresses the problems to be solved in Beaker construction, taking into account pottery technology available at the time, and the visible evidence in the Beakers themselves. A Beaker potter may be using techniques taught to him, or a skilled Beaker potter may see a Beaker brought in by trade or on his own travels and using his own skills fashions a similar vessel. Taking the study of Beaker construction methods further may help identify construction 'typologies' that can be integrated with existing typologies based on shape and decoration. Along with current Beaker study projects such as the Beaker Isotope Project, this could help in shaping our knowledge of the movement of the so called 'Beaker People'.

List of Examined Beakers

The Beakers examined for this study are listed first with their find locations, National Museum of Scotland reference number, Clarke's corpus number if applicable, and then the approximate height of the Beaker.

1. Bathgate, West Lothian; EG 53 (1789); 19 cm
2. Bathgate, West Lothian; EG 47 (1788); 14 cm
3. Aberdeenshire; EG 39 (1414); 12 cm
4. Brackmont Mill, Leuchars; EQ 720; 9.5 cm
5. Bailielands, Auchterarder; EG 40 (1738); 13.5 cm
6. King Street Road, Aberdeen; EG 35 (1421); 20 cm
7. Cruden, Aberdeenshire; EG 20 (1444); 17 cm
8. Cakemuir Hill, Midlothian; EG 12 (1707); 17 cm
9. Windy Mains, Haddington; EG 8 (1633); 16 cm
10. Littleton Castle, Kelso; EG 23 (1774); 17.5 cm to break

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8. Luminescence Dating of Medieval Brick from Essex: An Example of the Physical Sciences Addressing Archaeological Questions

Thomas Gurling

Abstract

This paper considers the role that science can play in archaeology, especially in the field of dating historic brick buildings. The important role that both science and experimentation play in archaeology is briefly considered before attention is given to the current approaches employed by archaeologists in the county of Essex for dating historic brickwork. These approaches are individually critiqued, highlighting the potential that exists for the application of the physics-based dating technique of optically stimulated luminescence (OSL). Through the application of these conventional dating approaches, an archaeological model has been created for Essex describing how brick was used during the medieval and Tudor period. A brief account outlining the use of brick during the 15th and 16th centuries according to this model is given before attention focuses on the brick manorial complex of Layer Marney Towers. A description of the current archaeological understanding of the collection of buildings that constitute this manorial complex is given, illustrating how certain conclusions can be drawn using conventional archaeological approaches. The implications that the luminescence date has on this understanding of the manor and suggested revisions are then described. Ultimately, this illustrates the value that a scientific dating technique offers in order to further the archaeological understanding of the site. Finally, consideration is given towards the value of adopting multi-disciplined approaches in archaeology and the way in which the direct scientific analysis of archaeological artefacts can be of benefit to both other forms of experimental archaeology and the archaeological theorist.

Scientific Study in Archaeology

Science has long played an important role in archaeology for much of the discipline's history. An early example occurred in the 1830s when Michael Faraday was involved in the chemical analysis of excavated Roman remains from the Bartlow barrows in Essex (Gage 1836, 306–310). Now, at the start of the 21st century there is a certain

level of debate surrounding exactly how archaeometrists and archaeologists should be involved in archaeological projects in the future (see for example Jones 2004 and the subsequent comments in *Archaeometry* 2005). However, many consider archaeology to be a broad discipline, rooted in the study of the material remains of past peoples through the collaboration of a wide range of disciplines that covers both natural and social sciences as well as the arts and humanitarian disciplines (Schroeder and Bray 2007, 11–12). Naturally, this form of archaeology incorporates the usage of a wide range of scientific disciplines in order to gain as much insight into the material remains recovered, including physics, chemistry, biology, geology, mathematics etc. (Pollard 1995; Tite 1991, 139–140).

Given the significant role that science plays in contemporary archaeology, it is worth highlighting the process in which it operates. This chiefly involves the recovery of material remains from a site before they are interpreted by the archaeologist. The interpretations are based on the current knowledge framework within the relevant field of archaeology. These knowledge frameworks are themselves based on a combination of the records of earlier excavations, ethnographic studies and contributions from the systematic study of recovered material remains. Such systematic study includes analysis by established scientific disciplines. Ultimately, it is possible to produce an interpretation of a site. This leads to the need for testing any interpretation or hypothesis formed about a specific interpretation of an archaeological period, site or material remains (Reynolds 1999, 157). Experimentation is one of the key means employed in the normal archaeological procedure to refute existing hypotheses and, where necessary, draw attention to the need to revise them (Malina 1983, 71). It is important to appreciate that experimentation can take many different forms and is often used at many different stages in the archaeological analysis of a site (Malina 1983, 71, 77). Several of the earlier articles in this volume involve the form of experimentation described as 'process and function' in which the aim is to determine how things were achieved in the past (Reynolds 1999, 159). However, another form of archaeological experimentation is the testing of scientific equipment in order to improve the acquisition of archaeological data (Reynolds 1999, 162). Whilst this was the approach that was adopted for the principal study of this paper, it should be noted that the various forms of experimentation used in archaeology are not exclusive but are complimentary and inter-dependent (Reynolds 1999, 158). Thus, the use of established scientific techniques can be used as a new means of acquiring data or to improve the current means by which data is obtained from archaeological material remains. This new data can result in existing interpretations being revised, leading to a subsequent need for further investigation in order to determine the validity and feasibility of these new interpretations. The use of experimental replication is one such way to address this need of testing revised interpretations.

Dating Historic Brickwork in Essex

The project upon which this paper is based focused on the scientific dating of historic brick structures, ranging from the 12th to the 16th centuries, in the county of Essex using OSL (Gurling 2006). The project itself is contributing towards the wider archaeological

need for absolute dates and chronologies for historic buildings of all social levels in order to provide archaeologists with a better understanding of the interpretation and meaning behind such buildings (Johnson 1998, 215). OSL has already been successfully applied to date historic brick structures in other parts of the country, including Lincolnshire (Bailiff 2007) and Suffolk (Antrobus 2004), illustrating the feasibility of using this science based dating tool.

The reason for selecting Essex was the fact that it is an area rich in historic brickwork dating to the medieval and Tudor periods, including structures which have long been thought to be some of the earliest examples of post-Roman brickwork in the country (Andrews 2005a, 142). It is also a region to which a great deal of archaeological attention has been given towards understanding the use and development of medieval and Tudor brick, resulting in a detailed chronological framework being formulated which describes how brick was employed as a building material (Ryan 1996). In order to derive this framework, several different archaeological dating approaches have been employed across the county. These include brick typologies, archaeological building analysis, documentary sources and some scientific dating approaches. Each of these approaches is critically discussed in more detail below.

Brick Typologies

The principle behind this approach is that the physical properties, such as dimensions, colour or fabric, of an individual brick or a portion of brickwork are compared to a series of bricks for which the provenance and date is known (Ryan and Andrews 1993). Another approach involves the analysis of specific features on the brick in question that are produced through the manufacturing techniques that were characteristic of certain historic periods (Campbell and Saint 2002; Harley 1974). The key advantages to such a dating approach include speed and ease of analysis as well as its cost effective nature. However, the approach requires a comparative brick typology for a specific region or period, which can take time to establish. There are also certain periods when the physical properties of brickwork altered very slowly, resulting in poor resolution when dating by this approach. An example of this can be seen in the red 'Tudor' type brickwork of the 15th century which is typologically similar in many respects to red 'Tudor' brickwork in the 16th century (Ryan and Andrews 1993, 94; Harley 1974, 74–75). In turn, this makes it difficult to detect brickwork that might have been re-used in later contexts. It has been proposed that the brick typology that exists in Essex allows brick to be dated to an accuracy of 50–100 years (Ryan and Andrews 1993, 93).

Architectural Analysis

A common approach to determining the date of brickwork found within a structure is that of architectural analysis. This involves two key stages. First, the different components of the building are phased, forming a relative chronology through the recognition of continuities and breaks in wall fabrics (Morriss 2000, 157–162). Once this relative chronology has been established, absolute dates are, where possible, allocated to different parts of the structure through the identification of diagnostic fittings, such as date plates, or decorative features (Hall 2005).

Whilst such a dating approach can potentially offer a relatively quick and cost-

effective approach of dating, it is dependent on several factors in order to derive an effective chronological sequence. This includes the rate at which architectural fashions changed in a particular region and diagnostic features being either present or observable within the building fabric (Brunskill 1992, 124–127). The fact that it is often only high status buildings which have such diagnostic architectural features incorporated into their fabric represents another potential limiting factor to this dating approach. The possibility of materials being re-used or of structural alterations being made to a building in later periods are further ways in which archaeologists can be misled by this dating approach (Laws 2003, 26). The use of date plates for deriving absolute dates for parts of a building can also be highly misleading, since the date plate itself might refer to a non-architecturally significant event, such as a change in ownership (Brunskill 1992, 128).

Documentary Evidence

Documentary evidence is a dating source that can potentially be highly informative and insightful with regards to how historic brickwork was used in the construction of a building. There are several sources that are available to the archaeologist, including building contracts, especially for royal building projects, licences to crenellate and bequests left by individuals in wills towards the construction of specific projects, such as church additions.

Such documentary evidence is highly valuable but has several limitations associated with it. First, it is a form of evidence that is rare for most historic medieval structures. Where it does occur, it is most often found for high status buildings, such as the late 15th century brick castle Kirby Muxloe, Leicestershire, which was built by Lord Hastings, a Yorkist Leader and favourite of Edward IV (Wight 1972, 132). Secondly, documentary accounts are not always reliable. This can be seen in licences to crenellate which were intended to illustrate the social status of the household rather than to represent official Royal licences for construction work to be undertaken on a building (Liddiard 2005, 44; Coulson 1993). Another limiting factor inherent to documentary sources is the fact that, even where they do provide a detailed and accurate account of the building works, there is no guarantee that there will be any details of later alterations to the building in the documentary record.

Scientific Dating Techniques

There are several scientific dating techniques currently available to archaeologists for dating historic brickwork. The most common of these techniques is dendrochronology, an approach that can potentially offer high precision for the felling date of trees (Morris 2000, 142–143). However, there are a number of factors that can prevent or limit a date being derived by dendrochronology. These include the lack of exterior bark, an insufficient number of tree rings or the absence of a master chronology for the type of wood being studied (Aitken 1990, 46–47). Ultimately, dendrochronology is an indirect means of dating brick since there is an underlying assumption that the date of the timber is the same as that of the historic brickwork.

There are other scientific dating techniques that can be used to directly date the brickwork itself, including archaeomagnetism. This approach commonly requires

that the brick be sampled in its original position since firing in the kiln, posing a serious problem for most brick buildings, although recent research focusing on the intensity component of the archaeomagnetic field offers the opportunity to refine and date brick that is not *in situ* (Casas *et al*. 2007). Luminescence, as mentioned earlier, is another approach that has been shown to be highly effective in directly dating historic brickwork (Bailiff 2007). Finally, radiocarbon dating is another method that can be used to try and determine a date for when a portion of brickwork was erected by dating the mortar component of the structure, although such work requires an awareness of several different contamination effects that can produce false dates (Lindroos 2007). With the exception of dendrochronology, none of these scientific techniques have been routinely used for dating historic brickwork in Essex.

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The above discussion serves to illustrate the various approaches that are available to archaeologists for dating historic brickwork. Often, it is largely non-scientific dating approaches (brick typologies, architectural analysis and documentary sources) that are used to date historic brickwork, with dendrochronology representing the most common scientific dating tool used (Morriss 2000, 142). Whilst such approaches do provide highly valuable information, they all have the fundamental caveat that they only date the brickwork indirectly. Nevertheless, it has still allowed a chronological model describing the development and usage of brick in Essex to be formulated and it is important to note that these approaches are continually used to revise and refine this model. An example of this can be seen in the brick church tower at Billericay. Originally, this had been dated to the late 15th century based on a bequest to the church dated to 1496 (Ryan 1996, 63). However, subsequent analysis of the building fabric revealed a series of decorative tiles which were stylistically dated to the third quarter of the 15th century, suggesting that the tower might actually be of an earlier date (Andrews 2005b, 167–168). Essex therefore presents the ideal opportunity to experimentally apply a scientific dating approach which dates the brick directly in order to test the current archaeological understanding of the development and use of historic brick during the medieval and Tudor periods.

Late Medieval Brickwork in Essex

Before considering the case study of Layer Marney Towers below, it is worth focusing attention briefly on the archaeological context to which this building relates. Specifically, this involves considering the use of brick as a building material during the 15th and early 16th centuries in Essex. The brick type in use during this period was the red 'Tudor' brick, a form that first appears in Essex at the start of the 15th century and was used through to the early 17th century (Andrews 2005a, 145; Ryan 1996, 47).

During the course of the 15th century, the red 'Tudor' bricks were largely used by the wealthy nobility to erect substantial manorial complexes, of which there are several surviving examples in Essex. The earliest is the Moot Hall, Maldon, a three storied

Figure 8.1. Maldon Moot Hall. This three storied town tower house is the oldest standing complete brick structure in Essex, dating to the first quarter of the 15th century.

tower house (see Figure 8.1). Originally part of a much larger manorial complex, the surviving building exhibits many of the advanced skills which the brick craftsmen of the time were capable of, including an impressive carved brick newel staircase, decoratively carved trefoil brickwork and traces of decorative ruddling (a decorative scheme whereby brick surfaces are covered in red ochre before the white mortar joints are highlighted to enhance the aesthetic impact) on the internal surfaces (Andrews 2007). Many of the brick craftsmen in the 15th century are thought to have originated from northern Europe, something that is evident at Nether Hall, a moated manorial complex erected in the 1450s and 1460s. Here, a break in one of the outer walls to the moated site indicates a change in the quality of the craftsmanship, attributed to a change in the workmen involved in erecting the building. The high quality brickwork has been attributed to foreign craftsmen whilst the remainder is attributed to English craftsmen who were still learning the art of brick production and construction (Andrews 2004, 94–96).

In the period between the end of the 15th century and the early 16th century, brick began to be used to a greater extent among the lower echelons of society. This is most evident in the architectural additions that were generally being made to local churches around this time (Morris 1989, 353–355). In Essex, such additions were often undertaken in brick and included towers, clerestories, porches or, occasionally, the entire church being built out of brick, as was the case with the church at Chignal Smealy (Ryan 1996, 71–73). Work was often funded through wealthy patrons, including local nobility and wealthy merchants, as well as bequests left by members of the local community (Ryan 1996, 71–73; Morris 1989, 355–356). Brick was just as fashionable among the nobility during the early 16th century, largely due to the building activities of Henry VIII. An example of his work in Essex can be found at New Hall, Boreham, where he constructed a palatial complex between 1516 and 1521 on the site of an earlier manor (Tuckwell 2006, 4–7). The early 16th century therefore saw a culture develop in Essex in which large brick manor houses were being built throughout the county, many by the 'new men' of Henry VIII's court who wished to extend or re-build an earlier, less fashionable manor houses (Andrews and Ryan 1999, 42; Ryan 1996, 74). It is amongst this group of structures erected by the 'new men' in the early 16th century that Layer Marney Towers belongs.

Application of Luminescence to Layer Marney Towers

The following case study serves to illustrate how a combination of both scientific and non-scientific analytical approaches can result in different archaeological conclusions being derived for an individual building. It also demonstrates how a greater amount of information can be obtained through a combination of both approaches. It focuses on the imposing gatehouse complex at Layer Marney, Essex (see Figure 8.2).

The gatehouse is a tall brick building and was originally intended to be the entrance to an impressive courtyard manorial complex. It is surrounded by several other historic brick structures including two side wings, a church, a southern 'gallery' range and a timber barn (see Figure 8.3). It has long been thought that large parts of the present manorial complex were erected in the early 16th century under Henry Marney, with

Figure 8.2. Layer Marney Towers. The gatehouse was originally intended to be part of a large courtyard house complex which was never finished.

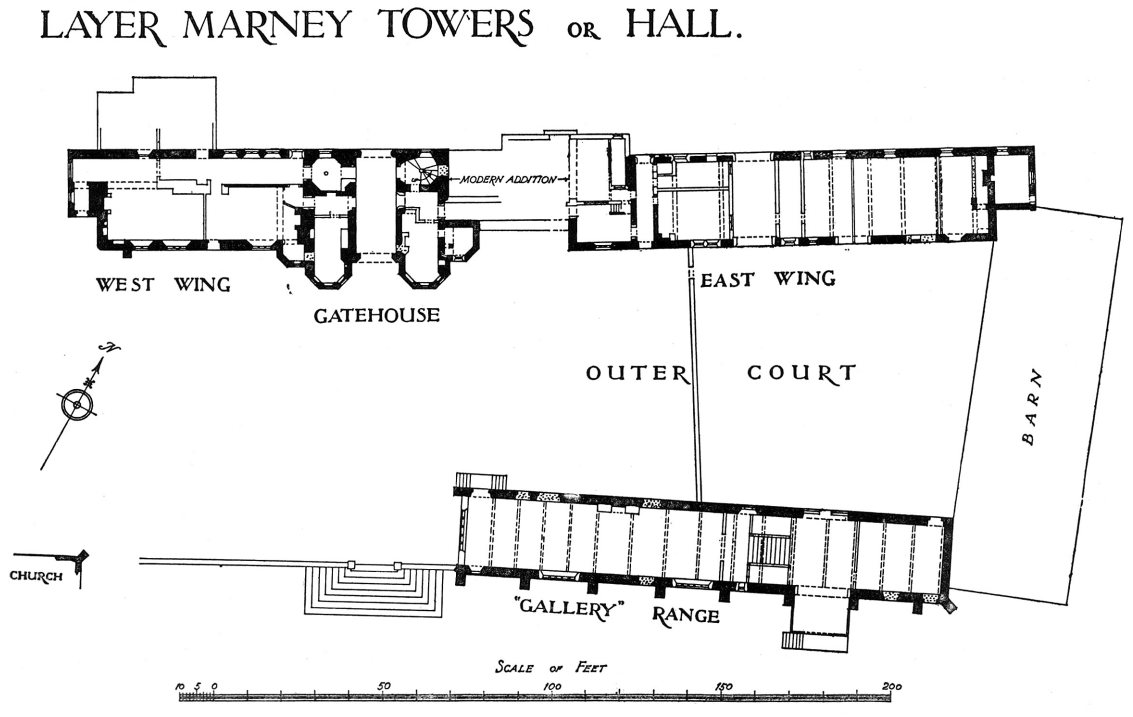

Figure 8.3. Layout of the principal buildings at Layer Marney. With the exception of the barn, all the structures are built principally from brick (note that only the corner of the church is shown in the diagram) (source: RCHME, 1922, 159).

work coming to a halt upon the death of his son John Marney in 1525 (Ryan 1996, 79; Andrews *et al*. 1986, 172; RCHME 1922, 157). This idea has been derived through several sources of evidence, including historic, architectural and documentary evidence, a summary of which is provided below.

Historically, Henry Marney had risen through the social hierarchy under the early Tudor kings during the late 15th and early 16th centuries. Under Henry VII he was made a privy councillor, an MP for the county of Essex and knighted. Henry VIII continued to appoint him to a number of other positions, including captain of the kings guard, electing him knight of the Garter in 1510, and promoting him to baronial status just prior to his death in 1523 (Carley 2004, 735–736). It is therefore likely that Henry Marney and his son had the resources to undertake a major construction project at Layer Marney in the early 16th century. As mentioned earlier, this would have also been in keeping with the general trend of the time in which royal courtiers were creating large, fashionable manorial complexes, many based around a central courtyard, in an attempt to emulate the building patterns of the monarchy and the royal court (Howard 1987, 24, 27).

Architecturally, it is evident that there was probably an earlier manorial complex on this site pre-dating some of the early 16th century buildings seen today. This includes an earlier church, the evidence for which was discovered in the early 20th century during restoration work which uncovered carved Romanesque stonework used as a rubble core infill in the church (Chancellor 1918, 65). The southern 'gallery' structure is also thought to be an earlier structure given its unusual alignment in relation to the other buildings, such as the eastern range (see Figure 8.3) (Bettley and Pevsner 2007, 529). It was also shown that there was an isolated window jamb at the western end, indicating that it had originally extended further westwards (RCHME 1922, 159). This would have partially obscured the view and approach to the central gatehouse suggesting it belongs to an earlier series of buildings. The barn has also recently been dated to the mid-15th century (Bettley and Pevsner 2007, 529). Other buildings at Layer Marney were almost certainly erected in the early 16th century, including the central gatehouse, west wing and the church. The presence of terracotta in the fabric of the central gatehouse and west wing is one source of architectural evidence which offers a precise dating range for these two structures. This is due to the fact that terracotta was briefly fashionable in England from c.1510 until the Reformation in the 1540s (Ryan 1996, 81; Wight 1972, 180).

With regards to documentary evidence, consultation of the wills of both Henry Marney (died 1523) and John Marney (died 1525) offer further clues that building work was underway at Layer Marney at the end of their lives. Henry Marney left instruction that the chapel adjoining the parish church which he had begun to construct was to be finished (both the chapel and church are made from red 'Tudor' brick) and that brick alms houses were to be built (an alms house is recorded as having stood close to a pond near the house) (King 1869, 150–151; Morant 1768, Vol. I, 409). John Marney left £200 towards the completion of the church and also refers to 'the newe galery on the west side of the tower' (King 1869, 157, 160), suggesting that this part of the manorial complex had recently been erected.

Based upon the above evidence, it is clear to see how many have come to the con-

clusion that both brick production and building work was underway at Layer Marney in the early 16th century. The presence of terracotta in the fabric of the gatehouse itself and reference to the adjoining west wing in John Marney's will also supports the idea that this part of the manorial complex was being erected in the 1520s. Based on this understanding of the building, there was an opportunity for a scientific dating approach to be employed in order to evaluate the current archaeological understanding of the building. A luminescence sample was collected from a brick in the south east turret of the central gatehouse for analysis. The result obtained was AD 1447±35, a value that is approximately 70 years earlier than the conventional age many have previously assigned to this building. The result suggests that the sampled brick had been produced at some point in the mid-15th century and was probably re-used in the construction of the current gatehouse complex. In light of this, the question arises as to where the brick originated. Given the fact that there is evidence for two manorial complexes at Layer Marney, it seems that the earlier one was being sequentially demolished and the material re-used in erecting the new buildings. The earlier manorial complex is most likely to belong to the first half of the 15th century, based on the luminescence result and the age of the timber barn. Further evidence to support this date range and to also offer a potential source for the use of brick at Layer Marney in the 15th century exists when we consider that Anne Marney, the daughter of Sir William Marney, the owner of the estate in the early 15th century, married Thomas Tyrell of East Horndon, Essex (Morant 1768, Vol. I, 406). The Tyrell family are thought to have been involved in the use of brick for building projects in the early 15th century (Ryan 1996, 51). It is possible that the idea of building in brick or possibly even the craftsmen used by the Tyrell family might have been exchanged or recommended between these two families.

There are a few documented cases across England indicating that brick was re-used in the 15th and 16th Centuries. One example that has parallels to Layer Marney involves Fulbroke Castle, Warwickshire, a structure built in brick and stone by John, Duke of Bedford, in the early 15th century. It had fallen into ruin by 1478 and was largely demolished by Sir William Compton who was granted permission by Henry VIII to use the material in his new house at Compton Wynyates, a brick structure that still survives (Fox 1945, 92). Studies at other sites with OSL have also indicated that brick was re-used in the post-Medieval era (Bailiff 2007, 846; Bailiff and Holland 2000, 618) and there is some archaeological evidence to support this at other sites in Essex, such as in the 15th century clerestory of Bocking church where a 12th century medieval 'great' brick was found re-used among the masonry (Andrews and Crouch 2001, 289). It is also important to note that there was a general culture of re-using other types of building material, such as stone, during the medieval period (Stocker 1990). Therefore, the case at Layer Marney can now be seen as contributing to the wider understanding of how brick was employed in Essex during the late medieval period, suggesting that brick from 15th century structures was being robbed for building new structures during the 16th century.

As has already been discussed, attributing a precise date to red 'Tudor' brick can be a difficult task for the non-scientific dating approaches often adopted by archaeologists. It can therefore also be difficult to successfully identify the re-use of this brick type. From a brick typological perspective, this task is difficult due to the inherent similarities

that exist between 15th and 16th century bricks. Architecturally diagnostic features can also struggle to identify the re-use of 'Tudor' brick and can result in misleading conclusions being derived. It is clear that decorative features inherent to the brickwork itself, such as brick bonding patterns, can easily be replicated in new structures, again preventing the identification of brick re-use. Equally, if a more modern structure was to be constructed using re-used materials, as was the case with Layer Marney, then it is likely that more fashionable, contemporary decorative features would be employed in the new structure. This casts another source of doubt on the reliability of such features as a means to date the brickwork associated with them. At Layer Marney this can be seen in the use of the fashionable material of terracotta in the windows and doorways of the early 16th century structures as opposed to the stone which had been adopted for the windows and doorways of the earlier 15th century buildings (RCHME 1922, 158–159). Finally, the few documentary records that describe brick being re-used from this period might only provide a small account of an activity that was probably much more commonplace.

The case of Layer Marney has illustrated the means by which a scientific dating approach, such as OSL, provided valuable additional archaeological information on how a building material was employed in the late medieval and Tudor periods. This in turn complemented the existing archaeological information of the site and allowed a more detailed understanding to be derived. The use of OSL to identify the re-use of red 'Tudor' brick at Layer Marney and other sites has also presented the opportunity to revise and update the current archaeological model that had been derived for the use of this building material during the 15th and 16th centuries.

Conclusion

The case of Layer Marney demonstrates the role that science based dating can play in the field of buildings archaeology today. On a broader scale, this example illustrates that, whilst the common methods currently used for analysing historic structures do provide a large amount of important and valuable information, the luminescence dating provided further information that had largely eluded any previous archaeological or historic accounts of Layer Marney. Furthermore, this allowed the chronological relationship between the two manorial complexes to be determined. On a broader scale, this case serves to illustrate the critical distinction between *what* events were undertaken at an archaeological site and exactly *how* they were executed. In this situation, the event was the construction of a new manorial complex and the means was the re-use of material from the earlier structures. It is also evident how certain information about historic brick buildings, namely the re-use of brick in later structures, could potentially be easily overlooked through the use of more conventional archaeological approaches to dating brickwork.

Science and experimentation are both critical aspects of contemporary archaeology, offering archaeologists the means to objectively critique, evaluate and revise theories in the different areas of the discipline. As mentioned before, there are several different ways in which experimentation can be applied in archaeology. All ultimately have the goal of enhancing our understanding of the archaeological record. Some of the

earlier articles in this volume have involved replication experimentation in order to confirm or falsify existing hypotheses in order to try and achieve this goal. This case discussed in this article illustrates another form in which experimentation, specifically the testing and application of scientific equipment in order to improve the acquisition of archaeological data, can be undertaken to improve our knowledge of archaeological remains. It is important to emphasise that the different approaches are not exclusive and that the greatest understanding of the past can only be derived through the close collaboration of these different approaches. It is possible to illustrate this using another example from Essex. At the sites of Pleshey Castle and King John's Hunting Lodge, Writtle, archaeologists have recovered highly unusual and rare late medieval chimney bricks which occur in two different forms. With the exception of these two sites, these forms of brick are not currently known to occur anywhere else. Samples of these unusual bricks were included in a scientific study which involved the use of Neutron Activation Analysis (NAA) to provenance pottery in Essex (Wickenden 2001). The results indicated the likely origin of these brick types within Essex. Such information demonstrates one manner in which science can provide archaeological information about historic brick. However, this information could potentially be taken further. It would provide experimental archaeologists investigating the likely production techniques with the knowledge of what source of clay to use in their experiments, thus reflecting the situation faced by the original medieval craftsmen. This in turn would enhance their understanding of the likely production methodologies. When combined, the knowledge of the provenance of the raw materials and the likely production methodologies would offer the archaeological theorist with invaluable information to formulate new archaeological models surrounding the craftsmanship of medieval brick production. Ultimately, it is only through such unified and multi-disciplinary approaches that a greater and more accurate understanding can be derived of the archaeological record, something that is surely the principle aim of all archaeologists today and therefore to be encouraged in future archaeological investigation.

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